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CONDUCTED BY

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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

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VOL. XXVII.—FOURTH SERIES.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . Admiratio generat quaestionem, quaestio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,  
Cur mare turgescat, pelago cur tantus amaror,  
Cur caput obscura Phœbus ferrugine condatur,  
Quid toties diros cogat flagrare cometas;  
Quid pariat nubes, veniant cur fulmina coelo,  
Quo micet igne Iris, superos quis conciat orbes  
Tam vario motu.”

*J. B. Pinelli ad Mazonium.*

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[FOURTH SERIES.]

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JANUARY 1864.

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- I. *On the Adequacy of Laplace's Explanation to account for the Discrepancy between the Computed and the Observed Velocity of Sound in Air and Gases.* By JOHN LE CONTE, M.D., Professor of Natural Philosophy in the South Carolina College\*.

NOTWITHSTANDING the acknowledged incompleteness and obscurity which characterizes his physical reasoning on this subject, it is now universally admitted that the true principles of the theory of the propagation of sound were first enunciated (in 1687) by the immortal author of the *Philosophiæ Naturalis Principia*. In the 43rd proposition of the second book of the *Principia*, Newton shows how a body vibrating in an elastic medium will propagate *pulses* through the medium. In the 47th proposition of the same book he attempts to investigate the exact nature of the motion of the vibrating particles, and concludes that the motion of each molecule necessarily follows the law of the oscillating pendulum. In the 48th proposition he establishes the fundamental principle in dynamics, that, in a medium whose elastic force is proportional to the condensation, the velocities of pulses must vary *directly* as the square root of the elastic force, and *inversely* as the square root of the density. In the 49th proposition of the same book Newton deduces the celebrated conclusion, that the *absolute velocity* with which such pulses are propagated in the air is equal to that which heavy bodies acquire by falling freely through *half the height of a homogeneous atmosphere*. And lastly, in the Scholium to the 50th

\* Communicated by the Author. *Note.*—This paper was completed and ready for publication in July 1861; its transmission to England has been delayed by the interruption of communication incident to the great revolutionary struggle in which we are engaged.

proposition, he compares calculation with observation, and vainly endeavours to explain the glaring discrepancy by assumptions which are both gratuitous and untenable.

The logical incompleteness of Newton's solution of the problem of the motion of sound was first assailed (in 1741) by Cramer of Geneva, who succeeded in showing that his reasoning applied equally to other modes of oscillation than that which was assumed. It is true this contradicted the enunciation of the 47th proposition of the second book of the *Principia*; "but it confirmed and extended all the general results of the demonstration; for it left even the velocity of sound unaltered, and thus showed that the velocity did not depend mechanically on the type of the oscillation"\*. But this, as well as other unsatisfactory points in Newton's theory, was first distinctly perceived and pointed out (in 1759) by the illustrious Lagrange, and replaced by an exact and rigorous investigation. But still the expression for the velocity of sound remained identical with that originally delivered in the 49th proposition of the second book of the *Principia*. It is obvious therefore that, although Newton's solution of this problem was *incomplete*, it nevertheless contained the true principles of this physical generalization. With a just appreciation of the importance of the step taken by the great legislator of physical science, the illustrious author of the *Mécanique Céleste* appropriately characterizes it: "*sa théorie, quoique imparfaite, est un monument de son génie*"†.

But notwithstanding the confirmation and extension which the Newtonian theory received from the mathematical labours of Lagrange and Euler, the obvious discrepancy between the velocity of sound as given by the physical theory and by direct experiment, and which had arrested the attention of Newton, remained a perplexing fact. Lagrange suggested that Mariotte's law, which assumes the pressure to be directly proportional to the density, might not be true; and that if we assume the pressure to vary as the  $\frac{2}{3}$  power of the density, the discordance would be explained. But he assigned no physical cause for this more rapid variation of the elastic force of the air‡. On the other hand, Euler considered that the deviation of fact from theory might possibly arise from an incorrectness of analysis in assuming the factor  $\left(\frac{dy}{dx}\right)^2 = 1$  in the equation previously to integra-

\* Whewell's 'History of the Inductive Sciences,' vol. ii. p. 315. London, 1837.

† *Mécanique Céleste*, vol. v. book 12. p. 95. Paris, 1825.

‡ Poisson, in the *Journal de l'Ecole Polytechnique*, vol. vii. (cahier 14) p. 325. Also Poisson's *Traité de Mécanique*, 2nd edit. vol. ii. pp. 716, 717. Paris, 1833.



tion\*. Indeed, as this assumption is necessary in order to render the equation integrable, and as it requires that the magnitude of the displacements of the molecules of air is *infinitely small* in comparison with the length of the wave, there certainly was some ground for suspecting the admissibility of the assumption, and consequently for doubting the rigorous exactness of the resulting expression for the velocity of sound. But the more rational and satisfactory explanation of this discrepancy was reserved for a later period, and may be considered the legitimate offspring of the great development of the science of thermotics which characterized the close of the last century. The bearing of these discoveries on the problem of the propagation of sound did not escape the sagacity of Laplace. The known extrication of heat during the condensation of gases, furnished this illustrious physicist with a force which is momentarily developed in the vibrations of a gas, and which, augmenting the *ratio* of the elasticity to the density, necessarily increases the *velocity* of the generated pulse. Accordingly, about the beginning of the present century, he suggested this *disengagement of heat* during the wave-motion as the true cause of the perplexing discrepancy. In the year 1807, M. Poisson first put Laplace's suggestion in a distinct mathematical form, in a remarkable "Memoir on the Theory of Sound"†. Finally, in 1816, Laplace himself announced the following theorem as the result of his physico-mathematical researches in relation to this problem; namely, "The velocity of sound is equal to the product of the velocity which is given by the Newtonian formula, by the square root of the ratio of the specific heat of air under constant pressure, to its specific heat under constant volume"‡.

Since this period, although a large number of the most distinguished physical philosophers have regarded this extension of the Newtonian theory as having exhausted the problem of the propagation of sound, yet great obscurity and uncertainty seems to have been attached to it by eminent English mathematicians and physicists. Some have unhesitatingly denied the necessity as well as the validity of Laplace's explanation of the discrepancy between theory and fact, and have endeavoured to deduce formulæ for the velocity of sound which dispense with the disengagement of heat during the wave-motion, and which nevertheless give results sufficiently accordant with experiment. While some have questioned the correctness of the mathematical pro-

\* Herschel's "Treatise on Sound," *Encyc. Metrop.* articles 55 and 67.

† "Mémoire sur la Théorie du Son," in the *Journal de l'Ecole Polytechnique*, vol. vii. (cahier 14) p. 360 *et seq.* Paris, 1808.

‡ *Annales de Chimie et de Physique*, vol. iii. p. 238. Paris, 1816. Also *Mécanique Céleste*, vol. v. book 12. pp. 96 and 123. Paris, 1825.

cesses which have been employed, others have assailed the *physical* aspects of his explanation, and attempted to show that it is entirely inadequate to account for the discordance between theory and observation.

It appears to me that the obscurity which marks many of the discussions relating to this problem arises from *two distinct causes*: namely, *first*, from a *misconception of the physical theory of Laplace and that of Poisson, which is substantially the same*; and *secondly*, from the *difficulties and obscurities which invest the mathematical theory of partial differential equations in their applications to physical questions*. I shall consider each of these causes separately.

1. Under the *first head* may be included the speculations of Winter, of Tredgold, of Farey, of Galbraith, and, more recently, of Blake, on the supposed correction for the *modulus of elasticity* of air, and on the presumed influence of *barometric pressure* and *latitude* on the velocity of sound\*. It is certainly a curious illustration of physical misconception, that it was not clearly perceived, as a simple *corollary* from the investigations of Newton, as well as those of Lagrange, Euler, Laplace, and Poisson, that, *ceteris paribus*, neither the *density of the air* nor the *intensity of gravity* have any influence on the theoretic velocity of sound. For, according to the Newtonian formula, the velocity of sound in air  $= \sqrt{gh}$ ,— $g$  being the velocity generated by terrestrial gravity in a mean solar second, and  $h$  the height of a homogeneous atmosphere. Now, as the value of  $h$  depends on the ratio of the density of mercury to the density of air, and as the *latter* varies, according to Mariotte's law, as the height of the barometric column, it is obvious that  $h$  remains *constant*, or is independent of barometric fluctuations. Again, as the value of  $g$  *increases*, the magnitude of  $h$  *diminishes*, and *vice versa*; so that the value of  $gh$  remains *constant in all latitudes, at all altitudes in the atmosphere, and consequently under all barometric pressures*, provided there is no change of *temperature*†. Even MM. Moll and Van

\* Winter, in Phil. Mag. S. 1. vol. xliii. p. 201 (1814). Tredgold, *ibid.* vol. lii. p. 214 (1818). Farey, *ibid.* vol. lxiv. p. 178 (1824). Galbraith, *ibid.* vol. lxvi. p. 109 (1825); also vol. lxviii. p. 219 (1826). Blake, in Silliman's American Journal of Science, 2nd series, vol. v. p. 372.

† The same result may be reached by *algebraic reasoning*. Let  $D$  = density of mercury,  $d$  = density of air, and  $b$  the height of mercury in the barometric column. We have  $h = \frac{bD}{d}$ . Hence

$$v = \sqrt{gh} = \sqrt{\frac{gbD}{d}}.$$

Now ( $D$  being *constant*), by Mariotte's law,  $d$  varies *directly* as  $b$ ; hence

Beek, in their account of the experiments made in Holland, seem to have overlooked this fact, as they evidently ascribe an influence to the variation of gravity with latitude\*. On the other hand, Dr. Simons, in a paper "On the Theoretic Velocity of Sound," written in 1829, clearly shows that the velocity is independent of the latitude, and consequently of the intensity of gravity†. Mr. Herapath likewise manifests a distinct appreciation of the grounds of this physical conclusion‡.

Under the same head must be included a more serious difficulty which has from time to time arisen in the minds of many excellent philosophers, in relation to the influence of the *heat* and *cold* developed by waves of *condensation* and *rarefaction* on the velocity of sound as deduced from the theory of Laplace. Passing by the singular idea of Winter, who thought that, if Laplace's view was correct, a sonorous body kept in a state of continual vibration in the air must finally render it *sensibly hot*§, I proceed to a brief consideration of the more serious objections which have been urged on this score. Prof. William Ritchie, in a paper presented to the Royal Society in January 1837, regards the hypothesis of Laplace as untenable, "from the fact that a *rarefied* wave advances through the air with the same velocity as a *condensed* wave, which would *not* be the case," he asserts, "if in either instance their progress were influenced by the heat evolved"||. More recently this difficulty has been revived by Prof. Challis, who contends that, if a sudden *condensation*, by developing heat, produces a *greater* elastic force, a sudden *rarefaction*, by lowering the temperature, must produce a *less* elastic force¶. This view has been endorsed by Prof. Potter, who insists that, by considering the *cold* produced by rarefaction, "Laplace's reasoning would have been equally available if the velocity of sound in air had proved one-sixth *less* than the theoretic velocity, instead of one-sixth *more*"\*\*.

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the value of the factor  $\frac{b}{d}$  remains *constant* amid the fluctuations of  $b$ . Again, since any variation in  $g$  must *equally influence the weight of the mercury in  $b$ , and the weight of the column of air which balances it*, it follows that, *cæteris paribus*, variations in  $g$  are *without influence* on the value of  $b$ . But, by Mariotte's law,  $d$  varies *directly* as the compressing force, or  $g$ ; hence the value of the factor  $\frac{g}{d}$  remains *constant* amid the variations of  $g$  with latitude and height. Consequently the value of  $\frac{gbD}{d} = gh$  is *invariable*.

\* Phil. Trans. for 1824, p. 446.

† Ibid. for 1830, p. 212.

‡ Mathematical Physics, vol. ii. p. 65. London, 1847.

§ Phil. Mag. S. 1. vol. xliii. p. 206 (1814).

|| Ibid. S. 3. vol. x. p. 220 (1837).

¶ Ibid. S. 3. vol. xxxii. p. 283 (1848).

\*\* Ibid. S. 4. vol. i. p. 101 (1851).



In the animated and instructive discussions which followed the enunciation of this objection, it was first satisfactorily shown by Prof. Airy, and subsequently by Rankine, Stokes, Haughton, and Bravais, that the whole difficulty grew out of a glaring misconception of the physical basis of Laplace's reasoning\*. Thus, the Astronomer Royal shows that "the velocity *does not* depend on the absolute pressure of the air in its normal state of density, but upon the *proportion* of the change of pressure to the change of density. This is increased by the suddenness of condensation in one part, which, when the elastic force is great, makes it still *greater*—and by the suddenness of rarefaction in another part, which, when the elastic force is small, makes it still *smaller*,—thus in both ways *increasing the change of pressure*." In like manner, Prof. Rankine clearly shows that Laplace's "investigation starts from the principle, known to be a fact, that when the density of a gas is changed, whether by compression or dilatation, its temperature changes also, and it does not assume a pressure proportional to the new density until it has had time to recover its original temperature. . . . The momentary variation of temperature being in the same direction with the variation of density, the momentary variation of pressure, whether positive or negative, is larger, as compared with the original pressure, than the variation of density as compared with the original density. . . . Now the velocity with which a disturbance of density is propagated is proportional to the square root, *not* of the total pressure divided by the total density, but of the *variation* of pressure divided by the *variation* of density. . . . It is therefore *greater* than the result of Newton's calculation,—and this, whether the disturbance is a condensation or a dilatation, or compounded of both." This physical reasoning obviously demonstrates, to use the language of Prof. Stokes, "that the *development of cold by sudden rarefaction* is as much an essential part of Laplace's explanation of the *increase* in the velocity of sound, as the development of *heat* by sudden condensation." It seems to me that a candid review of this discussion must convince every impartial physicist that this objection has its origin in a total *misconception* of the fundamental principles of Laplace's explanation, and that the views of this illustrious geometer, so far from having been in the slightest degree invalidated, have emerged from the controversy with greater distinctness of features in relation to the reality and truth of their general physical outlines.

2. The difficulties and uncertainties embraced under the

\* Airy, in *Phil. Mag.* S. 3. vol. xxxii. p. 343 (1848). Rankine, *ibid.* S. 4. vol. i. p. 226. Stokes and Haughton, *ibid.* vol. i. pp. 305 and 332 (1851). Bravais, in *Ann. de Chim. et de Phys.* 3 sér. vol. xxxiv. p. 82 (1852).

second head, originating in the different physical interpretations of the mathematical processes and their results, are of a more intractable character. Here we plunge into the quicksands of *equations of partial differentials, of discontinuous functions, and of integrals containing arbitrary functions*; the arbitrariness of which has a signification in the applications of the functions to physical questions. Many of the most interesting and important dynamical problems involve the consideration of the true signification of mathematical results which are known to have been reached by processes *which are not rigorously exact*. Many of the equations are utterly unmanageable and incapable of integration unless certain *assumptions* are made. Hence questions in relation to the *warrantableness* of such assumptions in particular cases are perpetually arising among the most eminent mathematicians. Such difficulties in the mathematical theory of sound have been sources of perplexity and controversy from the time of Lagrange and Euler to the present period. It is very questionable whether the vast amount of intellectual energy and analytical ingenuity recently displayed in the discussions of the various points bearing on this problem by Challis, Airy, Stokes, Moon, Rankine, Haughton, Potter, Earnshaw, and others (however instructive and important in other respects) has made any substantial contribution towards a clearer reconciliation of the physical with the mathematical aspects of the questions at issue. It is not my purpose to venture upon ground rendered historical by the labours of the greatest geometers of the present century. But I must insist that purely mathematical questions should be kept quite distinct from the physical considerations,—and that, *in problems of this character*, no deduction from analysis is worthy of confidence *which does not admit of a rational physical interpretation, capable of being tested by observation or experiment*.

Judged by this criterion, many of the anomalous and contradictory results which mathematicians have deduced from the discussion of the theory of sound must be placed in the domain of the *hypothetical*, until physical facts shall be produced in corroboration of their reality. As an illustration of this, I would call attention to the startling conclusion to which Mr. Robert Moon has been led by his analytical investigations, namely, “that waves of *rarefaction* are transmitted *more rapidly* than waves of *condensation*,”—and that, as in the production of sounds both kinds of waves are usually generated in immediate succession, “we should hear sounds *double* . . . if both kinds of disturbance were capable of affecting the human ear.” Inasmuch as such a result is contradicted by experience, the difficulty is summarily removed by assuming “*that the sensation of sound is produced by ærial rarefactions alone* ;” that the waves of *conden-*

sation are “suppressed by the ear;” and “that aërial rarefactions are alone capable of stimulating that organ in man”\*. Assuredly it would have been more reasonable to suspect the correctness of the approximate analysis by which such a result had been attained, than to have attempted the solution of the difficulty presented by its discordance with experience by making a gratuitous physiological assumption which, from the very nature of the case, is utterly incapable of being tested by observation.

But conclusions of a still more extraordinary character in relation to the propagation of sound have been deduced by the Rev. Samuel Earnshaw of Sheffield, from his recent and elaborate mathematical researches on this subject†. As they have an important bearing on Laplace’s explanation, I propose to notice them somewhat in detail. Like Euler, he at first thought that it was not admissible to assume the factor  $\left(\frac{dy}{dx}\right)^2 = 1$ ; but, to his astonishment, when he had succeeded in integrating the differential equation without the use of *approximative* assumptions, the theoretical velocity of sound remained the same as before. This led him to examine the problem of the propagation of waves in an elastic medium *ab initio*. He maintains that previous geometers have erroneously assumed elastic media to be *continuous*, whereas he looks upon them as consisting of “*particles separated by finite intervals*.” This is the fundamental peculiarity from which his mathematical investigation starts, and from which the anomalous results are presumed to flow as logical sequences. I shall briefly indicate some of the extraordinary conclusions to which his formulæ conduct.

a. “*That the atmosphere is capable of transmitting sound-waves with any degree of velocity from zero to infinity: . . . that there is no other limit to the velocity with which a violent sound is transmitted through the atmosphere, than that which the possibility of supplying a sufficient degree of force in its genesis may impose.*”

b. “*That a musical note of any pitch is transmissible with two different velocities; and that there are two waves for every note.*”

c. According to Mr. Earnshaw, the extreme range of velocity indicated in *a*, divides itself into *three distinct kinds* of waves, all propagated with different velocities—giving rise to what he designates the “*Triplicity of Sound*.” Assuming *v* to be the observed velocity of sound, he makes the following classification

\* Phil. Mag. S. 4. vol. xvi. p. 528 *et seq.* (1858).

† Ibid. S. 4. vol. xix. p. 449 *et seq.*; also vol. xx. pp. 37 and 186 *et seq.* (1860).



in relation to the *velocities* of the three kinds of waves:—

*w.* Minute waves, velocity varies from 0 to  $\frac{1}{2}v$ .

*x.* Ordinary waves, „ „ „  $\frac{1}{2}v$  to  $v$ .

*y.* Violent waves, „ „ „  $v$  to infinity.

I do not presume to offer an opinion in relation to the *legitimacy of the mathematical processes* by which these extraordinary results have been reached; but the *physical aspects* of the subject seem to warrant *two queries*. First, is it admissible to assume the *law of force*, according to which the molecules of the atmosphere act on each other, to be *that of the 4th power of the inverse distance*? The only reason given for this assumption is, that it is “the *lowest power* of the inverse distance which the *mathematical expressions themselves* would permit us to try.” Secondly, does not the fact that the analytical processes lead to *two* entirely distinct *types of waves* (*w* and *x* belonging to the circular type, and *y* to the *exponential* type) indicate that there must be some error in the assumptions of the mathematician?

Restricting ourselves to the general physical bearings of Mr. Earnshaw's deductions, it seems to me that the difficulties in the way of their admissibility are absolutely overwhelming. Is it not almost a *physical impossibility* that the *same elastic medium* in which a disturbance occupies ten or twenty minutes in reaching a point ten miles distant from the source of agitation, should, by the *mere increase in the violence of the genesis*, propagate it through the *same space* in an *infinitesimal* portion of time? In a medium (as the luminiferous æther) in which the elasticity is *enormous* as compared with the density, we may imagine a pulse to be propagated with incredible velocity; but that the *same elastic medium* should transmit waves varying in velocity, according to the violence of their genesis, from zero to infinity, is *scarcely conceivable*. It is true there may be nothing *à priori* improbable in the assumption that the velocity of sound *might be* related to the *violence* of the disturbance; but the fact that the analytical investigations conduct to such *extreme results as to set at nought all our physical conceptions*, originates a strong presumption that they belong to that class of *mathematical fictions* which have frequently sharpened the ingenuity and brightened the imagination of some of the most eminent geometers.

Nevertheless it will be granted that all such general objections as the foregoing must fall to the ground whenever the deductions of theory are found to be accurately accordant with experiments. Let us turn our attention to this aspect of the question. At first sight, as this theory demands an *indefinite range of velocity*, it would seem to be very readily susceptible of experimental verification. According to Mr. Earnshaw, however, sound-waves belonging to the *first class*, or *minute waves*, whose velocities

vary from 0 to  $\frac{1}{2}v$ , are *inaudible to human ears*, and are consequently removed from the province of the experimentalist. The two remaining classes of sound-waves, embracing velocities ranging from  $\frac{1}{2}v$  to infinity, or from 166·2 metres (545·3 feet) per second to infinity, are, according to the author, within the limits of experimental verification. It might be supposed that we have here a variation of velocity abundantly sufficient to test the truth of the theoretical results by means of the most casual observations. But we are told that, by a legitimate interpretation of the analytical formulæ, all *musical sounds* audible to men are transmitted with *sensibly the same velocity*—that all *ordinary sounds* belong to the same *type*, and that in both of these cases *the velocity does not differ sensibly from that admitted by philosophers*.

Nevertheless the author admits that, “if the theory here advanced be true, the report of fire-arms should travel faster than the human voice, and the crash of thunder faster than the report of a cannon.” With regard to *musical sounds*, we have the most satisfactory evidence that sounds of every *pitch* and *violence* are transmitted with precisely the *same velocity*. The experiments of MM. Biot, Bouvard, Malus, and Martin prove conclusively that the harmony of musical notes was not in the slightest degree deranged by transmission through the air contained in a pipe 951·25 metres (3120·96 feet) long\*. Mr. W. H. Besant mentions an experiment involving a severer test of the truth of this, inasmuch as the *distance* was much greater. “On a fine and still evening of June 1858, the *Messiah* was performed in a *tent*, and the Hallelujah Chorus was distinctly heard, *without loss of harmony*, at a distance of *two English miles*”†. As it is well known that the human ear appreciates, with the greatest nicety, the slightest *differences* in musical intervals, these facts may be considered as establishing the absolute *constancy* of the velocity of all aerial sound-waves embraced within the musical scale.

Inasmuch as the *range in the violence of the genesis* (upon which the variation in the *velocity* is assumed to depend) of musical sound-waves may not be considered *sufficiently extended* to afford a satisfactory test of this question, it may be interesting to investigate the evidence derived from *other kinds* of sound. Mr. Earnshaw thinks that there is sufficient reason to believe that it is a *fact* that *violent* sounds are transmitted *faster* than *gentle* ones. At the Meeting of the British Association at Leeds in 1858, he was able to bring forward but a single fact which seemed to support his theoretical deduction; viz., that during one of Parry’s voyages to the north, having occasion to make

\* *Mém. d’Arcueil*, vol. ii. p. 422.

† Treatise on Hydrostatics and Hydrodynamics. Foot note at page 187 (1859).

some experiments on sound, it was observed by persons stationed at a distance, that the report of the cannon reached them before they heard the command to fire; thus showing, as he thinks, that the sound of the gun's report outstripped the sound of the officer's voice. Subsequently, from the observations of Professor C. Montigny of Antwerp, in a memoir printed in the *Bulletins de l'Académie Royale de Belgique*, Mr. Earnshaw has been satisfied that the *thunder-clap* comes fairly under his *exponential type* of sound-waves, and that in such cases sound is transmitted with a velocity far greater than he had imagined. In one of the instances recorded by M. Montigny, "the report of a thunder-clap reached him after an interval of two seconds, which according to theory should have occupied more than fifteen seconds;" that is, its velocity was about *seven times as great* as that of the reports of the cannon used by experimentalists for determining the velocity of sound. Other cases of thunder-claps, less striking, but pointing towards the same conclusion, are recorded in the memoir\*.

It is proper to remark that inductions drawn from observations such as those last mentioned must be received with extreme caution. Strokes of lightning are sometimes *three, four, or even five English miles in length*†. The velocity of electricity being incomparably greater than that of sound, the thunder may be regarded as originating *at one and the same instant of time* along the whole of this extended and, frequently, irregular line of disturbance. The observer estimates one element for determining the velocity, viz. the *distance*, under the assumption that *the sound is generated at the point struck by the electricity*; while the probability is that the sound reached him by a *much shorter route*, from some *nearer portion* of the line of genesis. In fact this consideration is of itself sufficient to vitiate all inferences deduced from observations on thunder-claps.

In reference to the evidence derived from the anomalous observations of Capt. Parry's men, it is scarcely necessary to remark that, being entirely unsupported by confirmatory observations in other quarters, they can hardly be considered as invalidating the records of the most trustworthy experimentalists, which concur in establishing the general fact *that all sounds travel at the same rate*. As will be shown in the sequel, the experi-

\* Phil. Mag. S. 4. vol. xx. pp. 37, 38 (1860).

† M. Gay-Lussac, from observations in mountainous districts, ascertained that the striking distance of lightning frequently *exceeds three miles* (Quart. Journ. of Science for Oct. 1825, pp. 172, 173; from *Ann. de Chim.* vol. xxix. p. 105). Sturgeon concluded, from observations made at Woolwich, that he saw flashes nearly *six miles in length* (Phil. Mag. S. 3. vol. v. p. 421, 1834). In several instances I have estimated the length of strokes of lightning to be *at least four miles*.



ments of Dr. Olinthus Gregory prove that sounds produced by *bells, muskets, and cannon* are propagated with the same speed. Indeed, a candid and impartial examination of the evidence which has been adduced in relation to this point will, I think, warrant the assertion that Mr. Earnshaw *has failed to produce a single unexceptionable fact, or a single satisfactory observation, in verification of his theoretical deductions.*

Resuming the more immediate consideration of Laplace's explanation of the excess of the observed velocity of sound over that calculated by Newton, it should be borne in mind that the development of heat and cold by sudden condensation and rarefaction, is not a mere *hypothetical cause* evoked to account for the phenomena, but is a well-known *physical fact, proved by direct experiment.* Moreover, inasmuch as Laplace's formula is a rigorous deduction from this physical fact, "the only way," to use the language of Professor Stokes, "of escaping from the conclusion that the velocity of sound is really increased by the cause assigned, is to suppose that the heat produced by condensation passes away so rapidly by radiation, that the result is the same as though condensation and rarefaction were incapable of changing the temperature of air." Indeed, Professor Challis\* has been driven to the adoption of such an assumption: he asks, "May it not be that the developed heat (whether positive or negative) is carried off too quickly by radiation to affect the temperature of the fluid?" Again, the same mathematician contends that experiment only proves that heat is developed by condensation "when the fluid is confined within *narrow limits,*" and that it is an *unsupported hypothesis* to assume "that there is increase of temperature in fluid of *unlimited extent.*" Let us scrutinize this view more narrowly.

In the *first place*, it will not escape attention that, inasmuch as during the propagation of elastic pulses the condensations and rarefactions are necessarily *momentary*, it is *primâ facie* physically *improbable* that the heat developed should escape by radiation. The loss of heat by radiation requires *time*; so that the condensation of the aerial molecules in the transmission of sound-waves is performed precisely under the circumstances *most favourable* to the free and full production of the *whole effect* in question. It is obvious, that the condensations being so *momentary* as to afford no time for the escape of heat by radiation, an *unlimited* mass of air is really *better adapted* to give this cause its full influence, than any experiments on the compression of gases in close vessels. Furthermore, in the *second place*, Professor Stokes† has submitted this view to an elaborate analytical

\* Phil. Mag. S. 3. vol. xxxii. p. 283 (1848); S. 4. vol. i. p. 407 (1851).

† Ibid. S. 4. vol. i. pp. 306-317 (1851).



investigation, and has succeeded in proving that, during the propagation of sound-waves in the atmosphere, the compressions and expansions of the particles of the elastic fluid take place so rapidly, that there is not time for any appreciable transmission of heat between different particles; and that consequently the variations of the pressure and density are related to each other as they would be in a *non-conducting vessel*. In other words, he has shown that Professor Challis's supposition, that the developed heat is lost by radiation, is *untenable*, and that Laplace's view has a *real physical foundation*.

Hence it follows that the momentary disengagement of heat during the transmission of ærial sound-waves must be regarded as a really existing cause, a *vera causa*, which *must* act to *increase* the velocity above what it would be were there no such thermal change produced by the changes of density. Under this point of view, it is clear that any mathematical theory of sound which fails to take cognizance of the *development of heat* in the propagation of pulses in the atmosphere is, *primâ facie*, defective and imperfect. On this score, the formulæ expressing the theoretical velocity of sound in air, given by Challis, by Potter, and by Earnshaw, are *fatally assailable*. Perhaps the formula of Herapath is excluded from this category; for it seems to have been a deduction from his ingenious speculations on molecular physics, the basis of which rests upon *thermotic* considerations.

Looking upon Laplace's explanation as a *vera causa* acting in the *right direction*, the rules of philosophizing will not justify us in referring the observed effect to an unknown or imaginary cause, when a *real one* is at hand adequate to account for the whole effect. Consequently the *onus probandi* rests with those who reject his explanation, to show that it is *not adequate* to explain the *whole discrepancy* between theory and fact. This aspect of the question seems to have been partially recognized by Mr. Earnshaw. He sets forth his reasons for discarding Laplace's theory in the following words:—

“I think I may say that the requisite value of the coefficient (commonly denoted by  $k$ ) is much greater than Dalton's experiments warrant, and than what would have been conjectured *à priori* to be its value. In looking also at the determinations of its value, and also of the value of the velocity of sound, I am a little suspicious that modern experimentalists have suffered themselves to be biased by a desire to make experiment and theory agree. At any rate, if we compare experiments made since 1816, when Laplace announced his theorem for the correction of Newton's result, with those previously made, it is impossible not to notice a very sudden and startling change; and in the same spirit the value of  $k$  has been gradually growing in the hands of

experimentalists till it is now large enough really to justify the opinion which has been expressed, that to Laplace is due the honour of having completed the solution, which was begun in England, of the problem of the propagation of sound. And, to speak candidly, it must be confessed that Laplace's sagacious suggestion undoubtedly has the air of a *vera causa*, although it requires a larger development of heat by the sound-wave than seems probable. But its great defect, if I may be allowed to consider it defective, is that the result it gives does not come up to experiment. The theoretical velocity, after being amended by Laplace's suggestion, still falls short of the experimental velocity by 24 feet, if we take this last to be 1090 feet; and by 76 feet if we take the velocity of sound to be 1142 feet as determined by Derham, Flamsteed, Halley, and the Florentine Academicians. It should be remembered also that theory might *à priori* be expected to give a result exceeding, rather than falling short of, experiment; for theory assumes the elasticity and fluidity of the atmosphere to be perfect, and we have reason to think both are really in a slight degree imperfect; and this is not likely to accelerate, but rather to retard (if it at all affect) the propagation of sound-waves. Upon the whole, after considering the matter in as impartial a spirit as possible, candour obliges me to confess that Laplace's suggestion does not furnish a *sufficient* cause. I do not deny that it may be a cause; but it is not the whole. There is a cause, still unrevealed, for the defect of the theoretical velocity"\*.

The views advanced in the foregoing extract demand notice in relation to several points. 1. That the value of the coefficient *k* should have *augmented* with the increasing refinements of methods of experimentation, is precisely what might have been *à priori* expected; for it is obvious that all the errors arising from imperfections of methods and instruments tend to *depress* its value below its true amount. It is therefore hardly admissible to ascribe this fact to any *bias* on the part of the experimentalists. 2. The charge that the experimental velocity of sound underwent a "sudden and startling" *diminution* after the announcement of Laplace's suggestion, is *not supported* by historical facts. It is generally admitted by physicists, that the only *reliable* determinations of the velocity of sound made *anterior* to 1816, were those of the French Academicians in 1738, and of Benzenberg in 1809. The results in both of these cases, when reduced to the standard temperature, *do not differ sensibly* from the later determinations of the French and Dutch experimentalists. 3. How little the theory in question is amenable to the objection "that the result it gives does not come up to experiment," will

\* Phil. Mag. S. 4. vol. xix. p. 450 (1860).

be abundantly evident in the sequel, when the results of theory are compared with those of the most reliable experiments. 4. While Mr. Earnshaw denies the *sufficiency* of Laplace's suggestion, he admits that it is a *vera causa*, and yet he obtains a formula which professes to give *accurately* the velocity of sound, *without taking cognizance* of this *vera causa*, this development of heat during the transmission of aërial pulses! Assuredly, if this disengagement of heat is a *real cause* tending to *augment* the velocity of sound-waves (however *inadequate* to account for the *whole* discrepancy between theory and fact), it should certainly have constituted an *essential element* in his mathematical investigation. As far as I am able to understand, the *whole excess* of velocity given by his formula above that of Newton is *exclusively* ascribed to the circumstance that he has discarded the assumption of *continuity* in the medium, and substituted in its place the hypothesis of "particles separated by finite intervals." The *thermic changes* taking place during the propagation of aërial pulses are entirely *excluded* from the investigation. Hence he concludes, "Thus we see that the error committed in calculating the velocity of sound was not the leaving out the consideration of the development of heat, but the supposing the medium of air to be *continuous*."

I now proceed to the consideration of the *adequacy* of the various formulæ which have been devised for computing the velocity of sound, by a comparison of their results with those of the best experimental determinations. The Newtonian formula forms the basis of every expression for the theoretic velocity of sound which the ingenuity of mathematicians has elicited. Without exception, they all concur that Newton's velocity must be multiplied by the *square root of a certain factor*. And the question is, What is the *true value* of this factor? Hence it is evident that, in order to a correct comparison of the numerical results given by the several formulæ with experiment, it is necessary, as a preliminary step, to determine the true value of the quantity  $\sqrt{gh}$ , expressing the Newtonian velocity of sound in air. As both  $g$  and  $h$  are determinable solely by experiment, let us see what are their best-determined values.

And *first*, in relation to the value of  $h$ , or the *height of a homogenous atmosphere*. According to the admirable experiments of M. Regnault\*, *one litre* of mercury at Paris, 60 metres above the sea, at the temperature of *zero Centigrade*, weighs 13595.93 grammes. According to the same experimenter†, as corrected by M. Ritter‡ for a slight error of computation,

\* *Mémoires de l'Académie des Sciences*, vol. xxi. p. 162. Paris, 1847.

† *Ibid.* p. 157. Paris, 1847.

‡ *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève*, vol. xiii. p. 361.



one litre of pure dry air, at the same place and temperature, and under a pressure of 0.760 metre of mercury at 0° Centigrade, weighs 1.2932227 gramme. Hence it follows that at Paris, 60 metres above the sea, and under the physical conditions above indicated, mercury is 10513.2163 times as heavy as air. Consequently the height of a homogeneous atmosphere  $= h = 0.760 \text{ metre} \times 10513.2163 = 7990.044388 \text{ metres}$ , or 26214.52864 British standard feet.

Secondly, in relation to the value of  $g$  (the velocity generated by terrestrial gravity in a mean solar second of time), it is well known that it depends on the accuracy of the determination of the *length of the seconds' pendulum*. The suspicion of Newton, confirmed by the long-neglected experiments of Du Buat, extended and verified as they have been by the more recent and accurate investigations of Bessel, Poisson, and Baily\*, have established beyond a doubt that the old formula for the *reduction to a vacuum* is very defective, and by no means expresses the *whole correction* which ought to be applied—in short, that the *coefficient of buoyancy* heretofore used in these reductions was entirely *too small*, in that it took no account of the influence of the *mass of adhesive air which a moving body drags along with it*. The absolute amount of correction required for this cause cannot be determined by calculation, “but must, in every case, be ascertained by actual experiment.” The experimental investigation has been undertaken both by Bessel and by Baily, from which it appears that the amount of correction will “vary according to the *length, magnitude, weight, density, and figure* of the pendulum.” According to Bessel’s experiments, the old correction for this element must be multiplied by the factor 1.956 to obtain the true reduction. The more elaborate and direct experiments of Baily indicate that the value of this factor is subject to such variations, that no great degree of confidence in the exact number can be secured, unless the *length, magnitude, figure, construction, and mode of suspension* of the pendulums which are the subjects of comparison *are identical*. Fortunately the platinum sphere used by Mr. Baily in his experiments was sensibly identical in all these particulars with that employed by MM. Biot, Mathieu, and Bouvard for determining the length of the seconds’ pendulum at Paris. According to Baily’s experiments with *this sphere*, the old correction must be multiplied by the factor 1.881 in order to obtain the true reduction. Inasmuch as M. Bessel’s experiments

\* Newton’s *Principia*, Book 2. Prop. 27. Cor. 2. Du Buat’s *Principes d’Hydraulique*, 2nd ed. Paris, 1786. Bessel, in *Memoirs of Royal Academy of Sciences of Berlin* for 1826. Poisson, in *Mémoires de l’Académie des Sciences*, vol. xi. p. 521 *et seq.* Paris, 1832; also *Traité de Mécanique*, 2nd ed. vol. i. p. 363. Paris, 1833. And Baily, in *Phil. Trans.* for 1832, p. 399 *et seq.*

were not made with pendulums which were in these respects comparable with those used for the determination of the length of the seconds' pendulum, we are perhaps warranted in regarding Mr. Baily's factor as the more trustworthy. Bessel's factor gives a difference of 2.59, and Baily's 2.395 in the number of vibrations in a mean solar day above the old reduction numbers,—in each case the pendulum being assumed to make 86400 vibrations in a mean solar day\*. Hence it follows that by Bessel's factor M. Biot's seconds' pendulum was 0.0596 millimetre too short; by Baily's factor it was 0.0551 millimetre too short. Consequently the deduced value of  $g$  was 0.5881, or 0.5438 millimetre too small, according as the former or the latter correction is employed.

But there is another element in deducing the exact length of the seconds' pendulum (and by consequence the value of  $g$ ), in which the old reductions are erroneous; the error being in an opposite direction to the foregoing. Dr. Thomas Young pointed out, what M. Poisson subsequently more fully demonstrated, that the formula usually employed for *reduction to the level of the sea* is too great, in that it takes no cognizance of the attraction of subjacent land lying above the sea-level†. Assuming  $L$ =the length of the seconds' pendulum at the point of observation,  $L'$ =its length at a station  $H$  units of height lower, and  $R$ =the earth's radius for the latitude of the station, the old formula is

$$L' = L + L \cdot \frac{2H}{R};$$

whereas Poisson's investigations, according to Professor Miller‡, give us

$$L' = L + L \cdot \frac{1.32H}{R}$$

as the corrected formula. The experiments of MM. Biot, Mathieu, and Bouvard at the "Salle de la Méridienne" of the observatory of Paris, 70.25 metres above the sea, and in latitude  $48^{\circ} 50' 14''$  North, give us  $L=0.993844842$  metre§. It must be borne in mind that this number was obtained by the use of the old formula for *reduction to a vacuum*. The value of  $R$  for the above latitude=6365360 metres. The following Table, computed from the foregoing corrected data, furnishes results which may be regarded as the best which experimental science affords.

\* Vide Table, given by Baily in Phil. Trans. for 1832, p. 433.

† Young, in Phil. Trans. for 1819, p. 70 et seq. Poisson's *Traité de Mécanique*, 2nd ed. vol. i. p. 495. Paris, 1833.

‡ Phil. Trans. for 1856, p. 784.

§ Biot's *Traité Élémentaire d'Astronomie Physique*, 3rd ed. vol. ii. Table at p. 466. Paris, 1844.

Height in metres.	Old formulæ for vacuum and height.		Corrected formulæ for vacuum and height.			
			Using Bessel's factor.		Using Bailey's factor.	
	L in metres.	g in metres.	L in metres.	g in metres.	L in metres.	g in metres.
70.25	0.993844842	9.8088554	0.993904427	9.80944350	0.993899941	9.8093992
60.00	0.993848043	9.8088870	0.993906539	9.80946435	0.993902053	9.80942005
0.00	0.993866779	9.8090719	0.993918906	9.80958640	0.993914420	9.8095421

It will be observed that the numbers obtained by the corrected formulæ are *larger* than those by the old formulæ—the error by *excess* in the old formula for *height* not being sufficient in this case to compensate for the error by *deficiency* in the old formula for *reduction to a vacuum*.

With these revised physical constants we are able to obtain a more accurate value for the Newtonian velocity of sound. Assuming the result deduced from the use of Bailey's factor as the standard, it appears that the value of  $g$  at Paris, at Regnault's laboratory, 60 metres above the sea-level, is equal to 9.80942 metres, or 32.1837179 British standard feet. The value of  $h$  being, as we have shown, equal to 7990.044 metres, it follows that the Newtonian velocity of sound, or

$\sqrt{gh} = \sqrt{9.80942 \times 7990.044} = 279.9602$  metres, = 918.5212 English feet per sexagesimal second, at the temperature of 0° Centigrade.

As the ratio of the specific heat of air under *constant pressure*, to its specific heat under *constant volume*, denoted by the fraction  $\frac{c}{c'}$  or  $k$  in Laplace's formula, must be determined by actual experiment, different experimenters, as might have been anticipated, have obtained somewhat diverse results. Two experiments of the extended series executed by MM. De la Roche and Bérard in 1812\*, furnished Laplace with the data for deducing the numerical value of this ratio. According to this mathematician †, the *ratio* of the heat given out by air, when submitted to pressures  $p$  and  $p'$ , will be  $= \left(\frac{p'}{p}\right)^{\frac{c'}{c}} = \left(\frac{p'}{p}\right)^{\frac{1}{k}}$ . In the two experiments referred to,  $p' = 1005.8$  millims. and  $p = 740.5$  millims., and the *mean* ratio of the heat given out in the two cases = 1.2396.

Consequently we have  $1.2396 = \left(\frac{p'}{p}\right)^{\frac{1}{k}}$ , or  $1.2396 = \left(\frac{1005.8}{740.5}\right)^{\frac{1}{k}}$ .

Hence we have  $k = 1.42511$ . This number cannot be considered very accurate, from the fact that the distinguished experimenters neglected to free the air from *aqueous vapour*. From the experi-

\* *Ann. de Chim. et de Physique*, vol. lxxv. p. 72 et seq.

† *Mécanique Céleste*, vol. v. book 12. p. 128. Paris, 1825.



ments of MM. Clément and Desormes\*, Laplace found  $k=1.354$ ; and MM. Gay-Lussac and Welter, from their experiments, obtained  $k=1.3748$ †. More recently M. Masson has undertaken an elaborate series of experiments, following the method of Gay-Lussac and Welter, from which he deduces for air  $k=1.41$ ‡. Such experiments demand the utmost delicacy of manipulation, combined with all the refined resources of modern experimentalists; and perhaps for this reason we are justified in considering Masson's result as the most worthy of confidence. No mention has been made of the results obtained by M. Dulong§, inasmuch as they are *deduced* from Laplace's formula.

For purposes of comparison, I subjoin the following Table of the "Theoretical Velocity of Sound" in dry air at  $0^{\circ}$  Centigrade, as deduced from the various formulæ which have been devised to solve this interesting question. The column containing the values of  $k$  indicates the factor, the *square root* of which, multiplied into the Newtonian velocity, should give the *true velocity* of sound in air.

Date.	Authority.	Formula for velocity.	Value of $k$ .	Experimenter.	Velocity at $0^{\circ}$ Cent. in metres per second.
1687 1759	Newton and Lagrange ...	$v = \sqrt{gh}$	.....	.....	279.9602
1816	Laplace and Poisson ...	$v = \sqrt{gh} \cdot \sqrt{\frac{c}{c'}}$	1.42511	{ De la Roche and Bérard. }	334.211
"	"	" "	1.354	{ Clément and Desormes. }	325.766
"	"	" " "	1.3748	{ Gay-Lussac and Welter. }	328.2585
"	"	" " "	1.41	Masson.	332.4343
1824	Ivory	$v = \sqrt{gh} \cdot \sqrt{\frac{4}{3}}$	1.3333	.....	323.2706
1847	Herapath ¶	$v = \sqrt{gh} \cdot \sqrt{\sqrt{2}}$	1.41421	.....	332.9307
1849	Challis **	$v = \sqrt{gh} \cdot \sqrt{1 + \frac{4}{T^2}}$	1.40528	.....	331.8780
1851	Potter ††	$v = \sqrt{gh} \cdot \sqrt{\frac{3}{2}}$	1.5000	.....	342.8798
1860	Earnshaw ‡‡	$v = \sqrt{gh} \cdot \sqrt{\frac{15}{T^2}}$	1.51982	.....	345.1375

\* *Journal de Physique*, Nov. 1819, vol. lxxxix. p. 333 *et seq.*

† *Mécanique Céleste*, vol. v. book 12. pp. 125 & 127. Paris, 1825.

‡ Péclet's *Traité de Physique*, 4th ed. vol. i. p. 571. Paris, 1847. Also Daguin's *Traité de Physique*, vol. i. p. 884. Paris, 1855.

§ *Mémoires de l'Académie des Sciences*, vol. x. p. 147 *et seq.*

|| *Phil. Mag.* S. 1. vol. lxiii. p. 424 (1824); vol. lxvi. p. 13 (1825). Mr. Ivory seems to have deduced the factor  $\frac{4}{3}$  from Dr. Dalton's law.

¶ *Mathematical Physics*, vol. ii. p. 63. London, 1847.

\*\* *Phil. Mag.* S. 3. vol. xxxiv. p. 98 (Feb. 1849).

†† *Ibid.* S. 4. vol. i. p. 104 (Feb. 1851). ‡‡ *Ibid.* vol. xix. p. 455 (June 1860).

In order to compare the results given by the foregoing formulæ with those obtained from experiments on the velocity of sound in the air, it is necessary to reduce the latter to the standard temperature of zero of the Centigrade scale. The more accurate coefficient of expansion for air, given by the experiments of M. Regnault, renders it requisite to make *new reductions* for temperature in all the older observations. Consequently I have in some measure restricted the comparison to such experiments as furnish the data requisite for such new reductions. I have been unable to obtain the data for reducing the results of Benzenberg's experiments. The numbers in the Table relating to the experiments of MM. Myrbach and Stampfer, and of MM. Bravais and Martins, are given on the authority of Professor Rankine, who seems to have employed the corrected coefficient for the dilatation of air\*. Most of the observations are deficient in the hygrometric data for computing the influence of *aqueous vapour*. This correction is, however, usually very small, and becomes *insignificant* at very low temperatures—such as existed during the experiments of Parry and Foster. But the admirable experiments of MM. Moll, Van Beek, and Kuytenbrouwer (which are certainly in every respect the most reliable) furnish all the data requisite for the most rigorous comparison. The numbers relating to these experiments are taken from the Table given by Dr. Simons†, which is restricted to such of the observations in which the guns were fired exactly at the *same moment* at each station, and in which the *time* was correctly observed at *both extremities* of the base. The reductions for temperature and vapour have been computed by means of the well-known formula

$$v = v' \sqrt{\frac{1 - 0.378 \cdot \frac{f}{b}}{1 + 0.00367 \cdot t}}, \text{ in which}$$

$v$  = velocity in dry air at 0° Centigrade.

$v'$  = velocity given by observation.

$f$  = tension of vapour, measured by column of mercury at 0° Cent.

$b$  = height of barometer, measured by column of mercury at 0° Cent.

$t$  = temperature on the Centigrade scale.

\* Trans. of Royal Society of Edinburgh, vol. xx. pp. 167 & 168.

† Phil. Trans. for 1830, p. 211. The *mean* data furnished by *each* of the two days' observation have been taken, and the reduction calculated *separately*. The numerical errors in Dr. Simons's Table, which were pointed out by Professor Miller (Phil. Mag. S. 3. vol. xv. p. 1, 1839), have been corrected. No correction has been introduced for the influence of the *carbonic acid* of the air, as its effect must be *inappreciable*.

The results of these calculations are presented in the annexed Table.

Date.	Experimenters.	Sources of sound.	Distances in metres.	Mean temp. Cent.	Observed velocity in metres.	Velocity at 0° Cent. in metres.	elocity in dry air at 0° Cent.
1738.	Cassini, &c. ....	Cannon .....	28526-09955	6-25	337-187	333-3851	
1821.	Goldingham* .....	" .....	9005-7628	28-861	350-2552	333-0602	
"	" .....	" .....	4246-4883	26-278	344-8784	329-3621	
						331-875	
1822.	Arago† .....	Cannon .....	18612-51983	15-90	340-8886	331-3580	
"	" .....	" .....	" .....	17-75	344-5167	333-8154	
						332-5865	
1822.	Myrbach .....		9940-87			332-960	
1823.	Moll, Van Beek, &c. .	Cannon .....	17669-28	11-095	340-2558	333-5330	332-7485
	" .....	" .....	" .....	10-898	339-3722	332-7825	332-0749
							332-4117
1823.	Gregorys .....	Muskets .....	1097-260	- 2-78	333-506	335-2206	
"	" .....	Cannon .....	2438-360	+ 0-56	335-020	334-6760	
"	" .....	Bells .....	1005-821	1-67	335-884	334-8590	
"	" .....	Muskets .....	1097-260	7-22	337-610	333-2246	
"	" .....	Cannon .....	1996-400	15-00	338-092	329-1546	
"	" .....	{ Howitzers .. }	944-863	15-56	338-931	329-6498	
"	" .....	{ Mortars ... }					
"	" .....	{ Cannon ... }	4102-538	17-78	339-889	329-3148	
"	" .....	{ Mortars ... }					
"	" .....	{ Muskets ... }	2688-286	18-89	340-303	329-0870	
"	" .....	{ Cannon ... }				331-8983	
824 } and 825 }	Parry and Foster § ...	Cannon .....	3929-6818	-21-67	317-136	330-552	
"	" .....	" .....	" .....	-22-77	316-175	330-276	
"	" .....	" .....	" .....	-31-39	311-192	330-831	
"	" .....	" .....	" .....	-27-77	316-758	334-248	
"	" .....	" .....	" .....	-38-61	307-928	332-375	
"	" .....	" .....	" .....	-39-16	309-180	334-121	
"	" .....	" .....	" .....	-29-72	312-914	331-516	
"	" .....	" .....	" .....	+ 0-83	334-762	334-253	
						332-2715	
1844.	Bravais .....						332-37

\* Phil. Trans. for 1823, p. 96 *et seq.* The data are selected from 202 experiments made on *calm days*; 122 observations for the *longer* distance, and 80 for the *shorter*. The *mean value* is computed by attributing to each determination a weight proportional to the *distance* of the stations.

† "(Euvres de François Arago," *Mémoires Scientifiques*, vol. ii. p. 1 *et seq.* In these experiments the signals *were not made simultaneously* at the two extremities of the base. On the *second* day the sound of the cannon was readily heard by the observers at one station, but those at the other station heard only *one* out of eleven explosions.

‡ Phil. Mag. S. 1. vol. lxiii. p. 1 *et seq.* (June 1824).

§ Phil. Trans. for 1828, p. 97 *et seq.*



The results obtained from Dr. Gregory's experiments cannot be regarded as comparable in *accuracy* with some of the others; for the *distances* were too *small*, and he was not provided with adequate means of measuring with precision intervals of time to a *minute fraction of a second*. Nevertheless they are interesting as demonstrating that the *violence* of sounds *does not* exercise any *appreciable* influence on the *velocity*. The experiments of Drs. Moll and Van Beek, from the fact that the distance was *large*, that they employed an instrument capable of measuring intervals of *one hundredth of a second*, and that the signals were *strictly simultaneous*, unquestionably furnish the most trustworthy results. Assuming, therefore, the *mean* result of these experiments as the standard, the following Table will illustrate the accordance of the several theoretical formulæ with fact. In applying Laplace's formula, I have taken the value of *k* to be equal to 1.41, in accordance with the recent experiments of Masson.

Authority.	Theory.	Experiment.	Difference in metres.
	Velocity in metres per second.	Velocity in metres per second.	
Laplace .....	332.4343	332.4117	+ 0.0226
Ivory .....	323.2706	.....	- 9.1411
Herapath ...	332.9307	.....	+ 0.5190
Challis .....	331.8780	.....	- 0.5337
Potter .....	342.8798	.....	+ 10.4681
Earnshaw ...	345.1375	.....	+ 12.7258

With these revised data, it thus appears that Laplace's formula gives a velocity which *exceeds* that furnished by the most reliable experiments, by about 0.074 of an English foot. It is obvious, therefore, that what Mr. Earnshaw characterizes as the *great defect* of Laplace's explanation, viz. "that the result it gives does not come up to experiment," *fails to be sustained by facts*, and fully warrants an emphatic endorsement of Sir John Herschel's judgment, that "in inquiries of such delicacy, and where the effects of minute errors of experiment become so much magnified, it seems hardly candid to desire a more perfect coincidence"\*.

It will be observed that Mr. Earnshaw's formula (for *ordinary sounds*) gives a result which surpasses the experimental velocity by 41.75 English feet; and yet the chief merit claimed for his theory is, that it gives a velocity for sound "which agrees *accurately* with the experimental value"! With the results of

\* "Treatise on Sound" in *Encyc. Metrop.* Article 78.

modern and more accurate experiments before him, it is difficult to imagine why he should adopt Dr. Young's estimate, "that the *true velocity of sound is about 1130 feet (344.42 metres) per second.*" Assuredly it will scarcely be maintained that this is the *best-determined* value for the velocity of sound at the standard temperature of melting ice! And yet, after the warning he has given experimentalists, it might be hazardous to suppose that the mathematician has suffered himself "to be biased by a desire to make experiment and theory agree."

I now proceed to the consideration of evidence bearing on this question derived from quite a different quarter. The development and experimental verification of the "Dynamical Theory of Heat" has furnished an unexpected and independent confirmation of the *truth* as well as the *adequacy* of Laplace's explanation to account for the *whole excess* of the observed above the Newtonian velocity of sound. A few words will, I hope, place this in a clear light.

According to the experiments of Regnault\*, the coefficient of dilatation of pure air under a *constant pressure* of 0.760 metre of mercury and at 0° Centigrade is equal to  $0.00367 = \frac{1}{272.4795}$

of its volume for an increase of *one degree* Centigrade in temperature. Let us suppose a cylinder, the area of whose cross section equals *one square foot*, filled to the height of 272.4795 feet with a homogeneous column of pure air, under the conditions of pressure and temperature above indicated. The *volume* of air contained in the cylinder being 272.4795 cubic feet, its *weight* will be  $272.4795 \times 0.0807288 = 21.99694$  lbs. avoirdupois. If the temperature of this column of air be raised from 0° to 1° C.

it will expand  $\frac{1}{272.4795}$  of its volume, and will consequently *rise* in the cylinder through a space of *one foot*. In this expansion it will *lift* the weight of the entire atmosphere pressing on *one square foot* (or 2116.268 lbs. avoirdupois) through this distance, namely *one foot*. Again, according to the experiments of Regnault†, the specific heat of pure air under *constant pressure* is 0.2379 in relation to the specific heat of an equal weight of water. Hence the amount of heat required to increase the temperature of 21.99694 lbs. of air from 0° to 1° C. equals the quantity of heat necessary to raise the temperature of 21.99694  $\times 0.2379 = 5.233074$  lbs. of water from 0° to 1° C. Of this amount (according to the dynamical theory of heat) a *part* is

\* *Mémoires de l'Académie des Sciences*, vol. xxi. p. 73. Paris, 1847.

† *Comptes Rendus* for April 18, 1853; also *Phil. Mag. S. 4.* vol. v. pp. 481 (1853).

expended in expanding the column of air, or, which is the same thing, in *lifting* the pressure of the atmosphere on *one square foot* through a height of one foot, while the *remainder* remains *free* and augments the temperature of the mass of air. But, according to the experiments of J. P. Joule\*, the quantity of heat required to increase the temperature of *one pound* of water *one degree Centigrade* is equivalent to that expended in lifting 1389·6 lbs. through a space of *one foot*. Hence the amount of heat consumed in lifting 2116·268 lbs. (the atmospheric pressure on *one square foot*) through a height of one foot would augment the temperature of  $\frac{2116\cdot268}{1389\cdot6} = 1\cdot522933$  lbs. of water *one degree*

Centigrade, being the amount of heat *expended in expansion*. It is obvious that this last, *subtracted* from the *whole amount* of heat employed, gives the quantity of heat which would be required to raise the temperature of the same mass of air from 0° to 1° C., *in case it were prevented from expanding*, or kept under *constant volume*. Consequently we have the following numerical data in relation to the column of air under consideration:—

Heat expended under <i>constant pressure</i>	. .	= 5·233074
Heat expended in <i>expansion</i>	. . . . .	= 1·522933
Hence, heat required under <i>constant volume</i>		= 3·710141

Hence, adopting Laplace's notation, we have

$$k = \frac{c}{c'} = \frac{5\cdot233074}{3\cdot710141} = \frac{1}{0\cdot70898} = \frac{0\cdot2379}{0\cdot168666} = 1\cdot410478.$$

Substituting this value in Laplace's formula, we obtain  $v = 332\cdot4907$  metres per second, which, considering the delicacy of the experiments from which the physical data have been drawn, is as perfect an accordance with experiment as the most hyper-critical could demand. In illustration of this fact, it is only necessary to notice that, if the coefficient of dilatation of air be assumed = 0·00366666 *instead* of 0·00367 (the other elements of the computation remaining the same), the value of  $k$  comes out = 1·40995, and  $v = 332\cdot4288$  metres per second†.

In the foregoing reasoning I have avoided all *theoretical* considerations and other elements entering into the formulæ which have been deduced from the "Mechanical Theory of Heat," and

\* Phil. Trans for 1850, p. 83.

† In the above computation I have taken the *specific heat* of air = 0·2379. Some physicists use the number 0·2377; while M. Jamin (vide *Cours de Physique*, vol. ii. p. 370, Paris, 1859), who appears to have had access to the *details* of Regnault's experiments, gives the number 0·23741. The



have preferred to draw my conclusions exclusively from the physical data furnished by the experiments of Regnault and Joule. Prof. W. J. M. Rankine\*, by using the same experimental data, in connexion with certain views in relation to the dynamical theory of heat which are more or less *hypothetical*, has arrived at nearly the *same value* of  $k$ . He finds it equal to 1·4094; hence the velocity of sound by Laplace's formula would be 332·3636 metres per second. It is proper to add that several years ago Mr. Joule himself† deduced the same general result in regard to the verification (which his determination of the mechanical equivalent of heat furnished) of the truth of Laplace's explanation of the excess of the observed above the calculated velocity of sound. But the imperfect and inaccurate character of the physical data (especially in respect to the *specific heat* of air) which were at that time at his command did not enable him to submit the question to a severe numerical test. The fact that the development of the "Dynamical Theory of Heat" has cast so much light on the theory of the propagation of sound-waves in the atmosphere, is one among the many illustrations of the *interconnexion* of all branches of physical science.

From the discussion of the several points embraced in this paper, the following conclusions seem to be fairly and legitimately deducible; viz.—

1. That the *disengagement of heat* (in accordance with the suggestion of Laplace) during the propagation of aerial sound-waves

following comparison will show the influence on the values of  $k$  and  $v$  :—

Specific heat of air.	Value of $k$ .	Value of $v$ .
0·2379	1·410478	metres. 332·4907
0·2377	1·41097	332·548
0·23741	1·41168	332·632

If we assume Joule's *equivalent of heat* to be 1390, instead of 1389·6, foot-pounds for 1 degree Centigrade, the following numbers are obtained :—

Specific heat of air.	Value of $k$ .	Value of $v$ .
0·2379	1·4103	metres. 332·471
0·2377	1·4108	332·528
0·23741	1·4115	332·612

\* Phil. Mag. S. 4. vol. v. p. 485 (1853); also Transactions of the Royal Society of Edinburgh, vol. xx. p. 589.

† Phil. Mag. S. 3. vol. xxxi. p. 114 (1847).

is a *vera causa* tending to *augment* the velocity of sound above what it would be by the Newtonian formula.

2. That the accuracy of the *physical reasoning* upon which Laplace's formula is based *has not been invalidated* by the recent discussions on the mathematical theory of sound.

3. That, using the most recent and trustworthy experimental data, Laplace's formula is *fully adequate* to bring up Newton's velocity to the *observed or measured* velocity of sound.

4. That, making use of the data furnished by the most accurate thermo-dynamical experiments, the *entire adequacy* of Laplace's explanation to account for the *whole excess* of the observed velocity above that given by the formula of Newton and Lagrange is *confirmed* in the most complete and satisfactory manner.

Before bringing this paper to a close, it may be proper to add a few remarks in relation to the propagation of elastic pulses in *mixed gaseous media*, such as the atmosphere. Three possible cases present themselves: viz. (a) The vibrations of the particles of the mixed gases may be regarded as giving rise to such interferences and anomalous collisions as to prevent the regular transmission of pulses, and of course to interrupt the propagation of sound. (b) The several gases may be considered as *co-tenants* occupying the same space without interference, and consequently *each gas* propagating its own elastic pulse independently of the others with a velocity *inversely* proportional to the *square root of its density*. (c) And lastly, between these two extreme cases there is an intermediate one, in which a *mutual action* among the particles of the constituent gases is contemplated. Let us consider each of these cases separately.

a. Under the *first* head may perhaps be included the anomalous results which Sir John Leslie observed in the case of a bell sounded in a mixture of hydrogen and air. But as the atmosphere obviously *does not* come under this case, I shall dismiss it.

b. Some philosophers have thought that the aqueous vapour of the atmosphere propagates sound quite independently of its other gaseous constituents. The *duplication of sound* sometimes observed in warm damp weather has been considered as probably arising from a part of the sound being transmitted *quicker* than the rest by the aqueous vapour\*. If such is really the case, it is rather singular that the phenomenon is not *more frequently recognized*. It is desirable that more careful observations should be made in relation to this point *under circumstances precluding the possibility of duplication from echoes*. Moreover, the usual

\* Herschel's "Treatise on Sound," in *Encyc. Metrop.* art. 109. Also Herapath's 'Mathematical Physics,' vol. ii. p. 66. London, 1847.

formula for correcting the influence of aqueous vapour (which assumes the *mutual action* of the gases) gives results so accordant with those deduced from experiments in the Arctic regions (where the tension of vapour is almost nothing), as to afford a strong confirmation of the view that the aqueous vapour of the atmosphere *does not transmit pulses independently* of its other gaseous constituents. Be this as it may, there can scarcely be any doubt that the nitrogen and oxygen of the air *do not transmit sounds independently of each other*. For if such were the case, although the most usual and powerful sounds would be transmitted by the nitrogen, yet it is hardly *possible* that the slower and feebler pulses which would be propagated by the oxygen should entirely elude observation. Considering the particles of these two gases as completely and absolutely independent of each other, it is easy to submit the question to a numerical test. Bearing in mind that the velocity of sound in gases, *ceteris paribus*, is inversely proportional to the square root of their densities, and assuming the value of  $k$  for both nitrogen and oxygen to be the *same as for air* (the difference is *very small*), we have the following results.

Let

$v$  = velocity of sound in dry air at  $0^{\circ}\text{C}$ .

$v'$  = velocity of sound in nitrogen at  $0^{\circ}\text{C}$ .

$v''$  = velocity of sound in oxygen at  $0^{\circ}\text{C}$ .

$d'$  = density of nitrogen, air being *unity*.

$d''$  = density of oxygen, air being *unity*.

Then

$$v' = \frac{v}{\sqrt{d'}} = \frac{332^{\text{m}} \cdot 4117}{\sqrt{0.971346}} = 337.279 \text{ metres per second,}$$

and

$$v'' = \frac{v}{\sqrt{d''}} = \frac{332^{\text{m}} \cdot 4117}{\sqrt{1.105612}} = 316.137 \text{ metres per second.}$$

It thus appears that, if the nitrogen and oxygen of the air transmitted sounds independently of each other, at the end of *one second* the nitrogen sound would be more than 21 metres (69.36 feet) in advance of the oxygen sound; and after the lapse of ten seconds the former would be more than 211 metres ahead. As the ear can readily appreciate intervals between sounds when the distances traversed differ less than 30 metres, it is scarcely possible to imagine that the duplication of distant and violent sounds could have escaped the most ordinary observation, were they transmitted independently by the gaseous constituents of the atmosphere.

c. Hence most physical philosophers have considered the atmosphere as a mixture whose gaseous constituents exercise a mutual



action on each other in the transmission of elastic pulses, and that consequently sounds are propagated in it in the same manner and with the same velocity as if it were a *homogeneous gaseous medium* having the known physical properties of the mixture. In fact, so far as the propagation of sound is concerned, some such view is necessary in order to account for the fact that sounds are *not duplicated* when heard at great distances. The difficulty of conceiving of such a *mutual action* between the nitrogen and oxygen of the atmosphere, under the assumption that it is a *mechanical mixture*, has led Mr. Herapath\*, and perhaps others, to conclude that the air must be a *chemical compound*. And it must be confessed that the *physical evidence* pointing in this direction is much stronger than I had imagined previous to the present investigation. Let us look at this point more closely.

It is proper to premise that the proofs which chemists have adduced to show that the air is a mechanical mixture are mainly *negative*. They show that there are *no evidences* of chemical action between the constituents of the atmosphere; that there are *no indications* of chemical forces resisting the separation of the nitrogen and oxygen; that Graham's law of diffusion, which secures uniformity of constitution, is a *physical* law depending on the relative density of the gases, and having no relation to their *chemical* properties, &c. At first sight this seems to constitute a strong body of evidence in favour of the Daltonian view. But it must be recollected that in *liquid mixtures* we have every gradation of manifestation of chemical phenomena, from extreme chemical activity to cases in which it is difficult, if not impossible, to detect any evidence of such action. Moreover it is well known that the more recent investigations of Prof. Graham have demonstrated the existence of analogous *physical* laws governing the diffusion of *saline solutions*, in which Dalton's idea, that the diffusibility is referable exclusively to the *elasticity* or *self-repulsion* of the particles, is totally inadmissible. Indeed philosophers have for some time past recognized the necessity of modifying Dalton's views, in so far as to admit the existence of a *mutual molecular attraction* among the particles of the diffusing gases†. In fact the late Mr. Ivory, in speaking of Dalton's hypothesis "that *one gas is inelastic to all other gases*," says, "although it be intelligible in words, is incomprehensible by the understanding." Nevertheless, while physicists have thus been compelled to acknowledge the *mutual action of the elasticities of intermixed gases*, yet, in the absence of evidences of distinct

\* Mathematical Physics, vol. ii. p. 37. London, 1847. Also Mr. T. S. Thomson, *ibid.* vol. xxv. p. 54 (1844).

† Mr. Ivory, in *Phil. Mag.* S. 3. vol. xx. pp. 81, 197, 278 (1842). Also Mr. T. S. Thomson, *ibid.* vol. xxv. p. 54 (1844).

chemical action, but few have ventured to regard the atmosphere in any other light than a mechanical mixture. But if such an action exists among the particles of *dissimilar* gases during intermixture, it is a legitimate question whether there are any indications of the manifestation of *feeble chemical forces* not hitherto recognized by chemists. Let us see what bearing the *physical properties* of the atmosphere have on this question.

It is obvious that, the *densities* of the nitrogen and oxygen in relation to air being known, the *percentage* of these gases can be computed, *provided the volume of the mixture is equal to the sum of the volumes of the two constituents*, which is the assumption made by the theory of mechanical mixture. The validity of this assumption can be tested by comparing the results of calculation with those of the best analyses. The experiments of Regnault furnish the numbers in relation to density\*.

Let  $V$  = volume of oxygen,

$V'$  = volume of nitrogen,

$d$  = density of oxygen (air being unity) = 1.105612,

$d'$  = density of nitrogen (air being unity) = 0.971346,

we have

$$d \times V + d' \times V' = 100 \times 1.$$

Also

$$V + V' = 100.$$

Hence

$$V = \frac{100 - 100 \times d'}{d - d'} = \frac{2.8654}{0.134266} = 21.341 \text{ per cent.}$$

Similarly,

$$V' = \frac{78.659}{100.000} \quad ,,$$

The proportion by *weight* would be

$$\begin{array}{rcl} \text{Oxygen} & . & . & . & . & = & 23.595 \\ \text{Nitrogen} & . & . & . & . & = & 76.405 \\ & & & & & & \hline & & & & & & 100.000 \end{array}$$

The results of the *volumetric* method of analysis afford the best *test* of this question, not only because they are probably more *accurate*, but likewise because the analytical processes assume the *same condition of things* in relation to the consti-

\* *Mémoires de l'Académie des Sciences*, vol. xxi. pp. 138, 142, 145. The numbers expressing the *densities* of oxygen and nitrogen are slightly different from those given by Regnault in his memoir. This arises *principally* from an error which is made in the *reduction* of his *first* experiment (p. 136) for determining the exact weight of a given volume of air. Of course this error pervades all the densities which are related to it. I have carefully revised all Regnault's computations for the reductions, and have obtained the numbers employed in the text.

tments of the air as has been done in the foregoing computation. In other words, the volume of oxygen is determined by detonation with hydrogen; the nitrogen is estimated by the difference; in both cases the assumption being that *no alteration of volume* takes place in the separation of the two gases. Of course, in order to compare these results with those of calculation, the proportion of oxygen and nitrogen is estimated for air which has been *deprived* of other constituents. The following Table will place this in a clear light:—

By Lewy*, at New Granada.	By Bunsen†, at Marburg.	By Regnault ‡, at Paris.	By calculation.
By volume. Nitrogen = 21·023 Oxygen = 78·977	By volume. O = 20·964 N = 79·036	By volume. O = 20·93 N = 79·07	By volume. O = 21·341 N = 78·659
100·000	100·000	100·00	100·000

The difference between these numbers and those deduced from the density of the constituent gases (varying from 0·32 to 0·41 per cent. of the volume) seems *too great* to be accounted for by the errors of the chemical analyses. For it must be recollected that M. Lewy regards the improved volumetric method of MM. Regnault and Reiset (that employed in the above analyses) as reliable within less than  $\frac{1}{10000}$  of the volume, or 0·01 per cent.§ It is likewise too great to be ascribed to the known *variations* in the composition of the atmosphere; the *extreme* fluctuations in this respect scarcely ever amounting to 0·10 per cent. of the volume.

On the other hand, let us look at the results of the analysis of the air by the method of *weights*. This method, although not claiming such extreme *accuracy* as the preceding, is entirely independent of any hypothesis in regard to the volume of the gases composing the mixture, and is capable of giving both the oxygen and nitrogen *directly* by weighing. The mean results of the experiments of MM. Dumas and Boussingault at Paris furnish us with the following comparison:—

\* *Comptes Rendus*, September 29, 1851. Also in *Phil. Mag.* S. 4. vol. ii. p. 500 *et seq.* (1851).

† Bunsen's 'Gasometry,' translated by Roscoe, p. 78. London, 1857.

‡ Regnault's *Cours Élémentaire de Chimie*, 5th edit. vol. i. p. 147. Paris, 1859.

§ *Comptes Rendus*, September 29, 1851. Also *Phil. Mag.* S. 4. vol. ii. p. 501 (1851).



Experiment.	Calculation.
By weight. O = 23·01 N = 76·99	By weight. O = 23·595 N = 76·405
100·00	100·000

The discordance in this case being greater than in the former, suggests that possibly in both instances the discrepancy may be attributable to errors in the determinations of the *densities* of the gases. The annexed Table will test the validity of this suggestion. The densities used are—1st, the *mean* values as given by the experiments of Regnault; 2nd, the *maximum* values given by the same experimenter; and 3rd, the values given by the experiments of Dumas and Boussingault.

Calculated percentage of oxygen and nitrogen.					
Experiments of Regnault.				Experiments of Dumas.	
$d=1\cdot105612$ , $d'=0\cdot971346$ .		$d=1\cdot105636$ , $d'=0\cdot9715395$ .		$d=1\cdot1057$ ,	$d'=0\cdot972$ .
Volume. O = 21·341 N = 78·659	Weight. O = 23·595 N = 76·405	Volume. O = 21·224 N = 78·776	Weight. O = 23·466 N = 76·534	Volume. O = 20·942 N = 79·058	Weight. O = 23·156 N = 76·844
100·000	100·000	100·000	100·000	100·000	100·000
Experiment. O = 20·96 N = 79·04	Calculation. O = 23·174 N = 76·775	Experiment. O = 20·96 N = 79·04	Calculation. O = 23·174 N = 76·790	Experiment. O = 20·96 N = 79·04	Calculation. O = 23·175 N = 76·827
100·00	99·949	100·00	99·964	100·00	100·002
Calculation. O = 20·812 N = 79·261	Experiment. O = 23·01 N = 76·99	Calculation. O = 20·812 N = 79·245	Experiment. O = 23·01 N = 76·99	Calculation. O = 20·810 N = 79·208	Experiment. O = 23·01 N = 76·99
100·073	100·00	100·057	100·00	100·018	100·00

Taking the results obtained by the *volumetric method* as the test, it seems, by a comparison of the numbers in the foregoing Table, that the *densities* given by the experiments of Dumas and Boussingault furnish results more nearly accordant with experiment than those calculated from the *densities* afforded by the more recent experiments of Regnault. The *methods* of the latter experimentalist are so far superior to those of his predecessors, as to preclude the assumption that the densities given by the

earlier experiments *are to be preferred*. Hence we are led to look for an explanation of the discrepancy between experiment and calculation in the direction of a possible *change of volume* taking place during the mixture of the two gases. Assuming the values of the *mean* densities given by Regnault's experiments, a change of volume to the amount of only 0.0512 per cent. would account for the *whole discrepancy*! For my original equations now become

$$d \times V + d' \times V' = 99.9488 \times 1,$$

and

$$V + V' = 100.$$

Hence

$$V = \frac{99.9488 - 100 \times d'}{d - d'} = \frac{2.8142}{0.13426} = 20.96 \text{ per cent.}$$

Similarly,

$$V' = \frac{79.04}{100.00}$$

Or, otherwise, estimating the percentage in relation to the *mixture*, a diminution of 0.0527 per cent. in volume would be sufficient: thus

$$d \times V + d' \times V' = 100 \times 1,$$

and

$$V + V' = 100.0527.$$

Hence

$$V = \frac{100 - 100.0527 \times d'}{d - d'} = \frac{2.8142}{0.13426} = 20.96 \text{ per cent.}$$

Similarly,

$$V' = \frac{79.04}{100.00} \quad ,,$$

Consequently, if this view is correct, there should be a *diminution of volume* when oxygen and nitrogen are mixed in the proportions above indicated, to the extent of somewhat more than 0.05 per cent. A *direct* experimental determination of the correctness of this explanation is certainly within the limits of accuracy afforded by the refinements of modern methods\*. Its verification would fortify the view in relation to the *mutual action* of the gaseous constituents of the atmosphere, towards which we are led by acoustic phenomena.

Columbia, South Carolina,  
July 8, 1861.

\* The memorable experiment which Berthollet executed in the cellars of the Observatory at Paris is usually cited as a *direct* proof that *no change of volume* takes place by the mixture of hydrogen and carbonic acid. As a

## II. *On the Stereoscopic Relief in the Chimenti Pictures.*

By SIR DAVID BREWSTER, K.H., F.R.S.

To William Francis, Esq., Ph.D.

DEAR SIR,

MY attention has just been called to the following statement by Professor Edwin Emerson, of Troy University, U.S., published in the *Philosophical Magazine* for February 1863.

"To prove this [that the stereoscopic qualities of the Chimenti pictures are 'evidently accidental'], let anyone execute a pen-and-ink sketch, and then let him make as perfect a copy of it as he can without careful measurements: now place these two drawings in the stereoscope, and *you get the same kind of effect seen in the Chimenti drawings, and for the same reason.*"

This appeal to *any one* for a scientific truth is rather an unusual mode of ascertaining it. Has Professor Emerson himself executed and copied any such sketch, and obtained the result which he assumes; and if he has, why has he not distinctly published the fact, and stated the nature of the sketch, and the precise *kind of relief* which he obtained. Had he done this, we might have challenged the accuracy of his experiment, and suggested a better mode of arriving at the truth.

In the absence of this information, however, we beg to ask him if he made a correct copy of *one* or *other* of the Chimenti drawings; and if he did, why he did not advise *any one* to do the same in order to obtain the only satisfactory *proof* of his assertion, that the relief in the Chimenti pictures is accidental.

Now we beg to tell Professor Emerson that we took this very method, and the only judicious one, of arriving at the truth. We went to one of the masters of the School of Art under the Board of Trustees in Edinburgh, with whom we were not personally

very slight variation of *atmospheric pressure* at the conclusion of an experiment extending over a long period of time would vitiate any deduction relating to a point of such *extreme delicacy*, may we not legitimately question whether *all the precautions* necessary for securing a *reliable result* were observed in this case? Moreover Regnault's recent experiments in relation to the "Elastic Forces of Vapours *in vacuo* and in Gases," while they confirm Dalton's law as a *theoretical law*, do not appear to verify it as an *absolute physical fact*—probably from the circumstance that it is impossible to find any two vapours or gases entirely deprived of all *reciprocal action*. Again, the researches of Bunsen ('Gasometry,' translated by Roscoe, London, 1857, p. 216) seem to show that the diffusive interchange between gases "does not occur in the relation of the inverse square roots of the specific gravities," and consequently that Graham's law is only *approximately* correct.

*Phil. Mag.* S. 4. Vol. 27. No. 179. Jan. 1864.

D



acquainted, and requested him to obtain from his pupils copies of each of the Chimenti drawings. We were thus furnished with *six* copies of one of the Chimenti figures by a pupil\* of the School, which could not produce, by any combination of them in the stereoscope, the effect of the Chimenti drawings. This was the opinion of Professor Tait, who mentions the experiment in the Number of the Photographic Journal referred to by Professor Emerson, who ought in fairness to have told his readers that the very experiment which he maintained could be made by *any one* with a *certain result*, had been already carefully made with *the very opposite result*.

If Professor Emerson made the experiment himself, or had it made by others, why has he not told us the *precise effect* that was produced? If it was only "*the same kind of effect*," as in the Chimenti drawings, and not the *same degree of effect*, the experiment was worthless. It might have been a stereoscopic leg, or a stereoscopic arm, or a stereoscopic shoulder, or a stereoscopic knee, or any other "*kind of stereoscopic effect*," without having any resemblance to the *actual degree of stereoscopic effect* produced by the Chimenti drawings.

As this question must be decided by experiment, we defy Professor Emerson to produce *one* result in a *hundred* sketches by different individuals in which the stereoscopic relief exists at all, instead of being as perfect as in the drawings alluded to; and if he could produce this miraculous sketch, it would merely prove that it was 99 chances to 1 that the stereoscopic relief produced by Chimenti was *not accidental*.

But if Professor Emerson were to obtain many examples of the production of relief by blunders in copying a drawing, this would only prove that the relief we have been considering *might be* thus produced, not that *it was* thus produced. An important question in the history of science is not to be settled by such poor logic as this. Many points must be considered before it is proved that what *might be* produced actually *was* produced.

The stereoscopic pictures have been preserved as the work of Chimenti himself, and the historian of science will naturally ask what led so eminent an artist to make so uninteresting a sketch

\* We had the sketches made by a pupil, to meet the gratuitous statement that one of the Chimenti figures *might have been* a copy by one of his pupils. It certainly might have been; but the drawings have been handed down to us as Chimenti's, and we are not entitled to assume, for any particular purpose, that *one* of them is the work of a pupil. The existence of two such drawings exactly similar to the eye, and placed as they are, is a circumstance so remarkable, that we venture to say that no similar pair of figures will be found, either in juxtaposition or singly, among the thousands of drawings left by ancient and modern artists.

as that of a man sitting upon a stool with a pair of compasses in one hand and a string in the other. He will ask what led him to make a copy of it of the very same size, and without any change or improvement whatever. He will ask what led him to preserve such uninteresting sketches; but, above all, he will ask, with a peculiar interest, what led him to *place these two figures side by side*, and in such a manner that the instant they were seen by an intelligent traveller, Dr. Crum Brown, he conceived that they were intended to be brought into relief by binocular vision, and actually brought them into stereoscopic relief by retiring and combining the two separate pictures. The historian of science will not be satisfied even with the obvious answer to these questions. He will inquire into the *date* of these drawings, and he will be struck with the fact that they were executed at the time when Baptista Porta had called the attention of philosophers to the subject of binocular vision, and had shown in a diagram so far resembling a stereoscope slide, that external objects, that is objects in relief, were seen binocularly by the combination of a right- and left-eye picture of them. He will also connect with these facts, though he may not yet give it much weight, the statement of Professor Archer, the distinguished Director of the Industrial Museum of Scotland, "that just before he left Liverpool, in a turn-out of the Museum there, *what appeared to be a stereoscope was found*, bearing the date of 1670. Being out of order," he adds, "it was laid aside at the time, and probably had not been touched since. It appeared, he said, to be of *Roman* manufacture; and he suggested that an attempt should be made to get it for examination, as having an important bearing on the subject before the Society" \*, namely the subject of Chimenti's stereoscopic figures.

In forming his opinion from these various considerations, the historian of science will not fail to notice that the binocular theory of Baptista Porta was published in *Italy* in 1593; that the stereoscopic drawings which so wonderfully illustrate it were also made in *Italy* some time after this, between 1620 and 1640, and that the probable stereoscope in Liverpool, with the date of 1670, is considered to be of *Italian* origin.

I am,

Ever most truly yours,

D. BREWSTER.

Allerly, November 27, 1863.

\* Photographic Journal, vol. viii. p. 12.

### III. *On some Effects produced by a Fluid in Motion.*

By GEORGE F. RODWELL, F.C.S.\*

IN the Philosophical Magazine for January 1851, Professor Magnus has given an account of the "Trompe," or Water-bellows, in which a continuous blast of air is produced by a stream of water, passing down the axis of a vertical tube, of greater diameter than the stream, and in free communication with the atmosphere, by orifices placed just below, or on a level with, the orifice from which the stream issues.

According to Magnus, air is carried down by the stream owing to the formation of concavities, which close and entrap air within them, and which are produced at the point where collision between the stream and a surface of water takes place.

The following experiments were made to determine the quantity of air carried down by a known quantity of water.

The water used in the experiments detailed below was supplied by a cistern 3 feet deep, the bottom of which was  $5\frac{1}{2}$  feet from the ground.

I. A glass tube A, fig. 1,  $\frac{11}{30}$ ths of an inch diameter, was bent as shown in the figure, so that its length from the upper orifice to the bend was 14 inches, and the length of the bent portion 2 inches; it was firmly fixed in a vertical position, and the curved end placed in a vessel B containing water to a depth of 3 inches.

A tube C communicating with the water-cistern,  $\frac{3}{20}$ ths of an inch diameter, was placed so that its orifice was level with that of the tube A, an

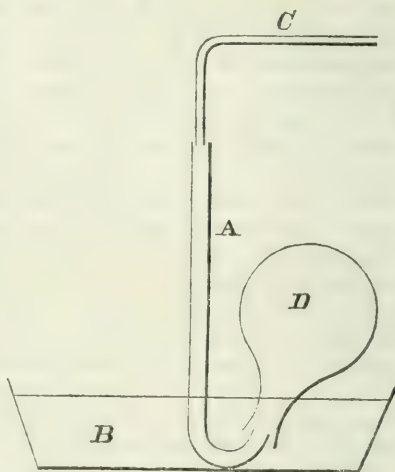
open annular space being left between the two tubes.

C delivered half a litre of water in 20 seconds, and the jet broke up at 8 inches from the orifice. The stream was allowed to flow down the axis of the tube A, and the air collected in a half-litre flask D, previously filled with water, and inverted over the lower orifice of A.

A violent foaming attended with a kind of roaring took place in A, at the point where the descending stream came into col-

\* Communicated by the Author.

Fig. 1.





lision with the water in the tube ; and by reducing the diameter of the lower orifice of A, so as to diminish the efflux, the collision could be made to take place at any distance from the orifice of C.

Before diminishing the efflux, the collision occurred at 8 inches from C.

					Half a litre of air was carried down in	
Collision	8 inches	from the orifice	of C	.	0 min.	50 secs.
"	6	"	"	"	1 "	2 "
"	1	"	"	"	2 "	15 "
"	$0\frac{1}{3}$	"	"	"	4 "	45 "
"	$0\frac{1}{4}$	"	"	"	no air carried down.	

II. A tube  $\frac{3}{20}$ ths of an inch diameter, drawn out in the form of a converging cone, so that its orifice was  $\frac{3}{40}$ ths of an inch diameter, was substituted for the delivery-tube C: it delivered half a litre of water in 33 seconds, and the stream broke up at 8 inches from the orifice.

Before diminishing the efflux from A, the stream struck against the curved part of the tube, and half a litre of air was carried down in 4 minutes 35 seconds.

					Half a litre of air was carried down in	
Collision	10 inches	from orifice	of delivery-tube		0 min.	46 sec.
"	7	"	"	"	0 "	46 "
"	5	"	"	"	0 "	52 "
"	3	"	"	"	1 "	10 "
"	2	"	"	"	1 "	31 "
"	$0\frac{1}{2}$	"	"	"	2 "	7 "

III. A tube  $\frac{3}{20}$ ths of an inch diameter, drawn out in the form of a converging cone, so that its orifice was  $\frac{7}{20}$ ths of an inch diameter, was substituted for the delivery-tube used in the preceding experiment: it delivered half a litre of water in 68 seconds, and the stream broke up at 12 inches from the orifice.

Before diminishing the efflux from A, the stream struck against the curved part of the tube, and half a litre of air came over in 6 minutes 40 seconds.

					Half a litre of air was carried down in	
Collision	10 inches	from orifice	of delivery-tube		1 min.	55 sec.
"	7	"	"	"	2 "	46 "
"	4	"	"	"	3 "	12 "

IV. A tube 20 inches long and  $\frac{3}{20}$ ths of an inch diameter, curved at its lower end, was substituted for the tube A, and a stream of water flowing from the delivery-tube used in Experiment II., at the rate of half a litre in 33 seconds, was allowed to flow down the axis of the tube.

Before diminishing the efflux from the vertical tube, collision

occurred at 8 inches from the orifice of the delivery-tube, and half a litre of air was carried down in 1 minute 7 seconds.

Half a litre of air was  
carried down in

Collision 5 inches from orifice of delivery-tube	2 min. 10 sec.
"    3    "    "    "    "    "	3    "    12    "

V. The delivery-tube used in Experiment III., the stream flowing at the rate of half a litre in 68 seconds, was substituted for the delivery-tube used in the last experiment.

Before diminishing the efflux from the vertical tube, the collision took place at 14 inches from the orifice of the delivery-tube, and half a litre of air was carried down in 2 minutes 6 seconds.

Half a litre of air was  
carried down in

Collision 7 inches from orifice of delivery-tube	2 min. 25 sec.
"    2    "    "    "    "    "	4    "    45    "

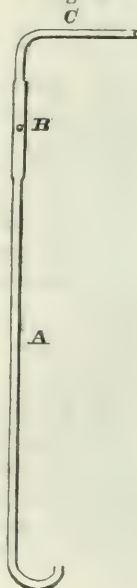
From the above experiments it is evident that the quantity of air carried down by a stream of water is greatest when the collision of the stream with water takes place at the point where the stream breaks up, and gradually diminishes as the distance between the orifice from which the stream issues and the point of collision decreases.

The effect of allowing a stream of water to flow through a vertical tube of the same diameter as the stream, and the upper part of which freely communicates with the atmosphere, was next tried.

To the upper orifice of a tube 18 inches long and  $\frac{3}{20}$ ths of an inch diameter (A, fig. 2), a piece of caoutchouc tubing, B, having four small orifices in its circumference, was adapted: the delivery-tube C made use of in Experiment I. was connected with the upper part of the caoutchouc, so that the orifices in the caoutchouc were midway between the orifices of C and A. The lower end of A was curved, and dipped  $3\frac{1}{2}$  inches below the surface of water.

When C was caused to deliver its maximum quantity of half a litre of water in 20 seconds, the stream flowed down the tube into the vessel below, exactly as it would have done if the upper part of A had not communicated with the atmosphere; but when the flow was diminished, air began to enter, and was carried down by the stream; on further diminishing the flow, the column in the tube assumed a peculiar banded appearance, and a base note was produced, which

Fig. 2.



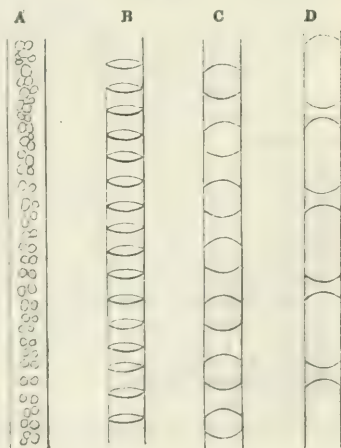
became more and more base as the flow was diminished, until distinct impulses were heard.

Suppose the orifices in B closed, and the tube A filled with water, it is evident that the column of water in A would have no tendency to fall, but would remain suspended in the tube, but directly the orifices were opened the column in A would fall, and air rush in to supply its place in the tube. Now when water is rapidly entering by the tube C, the particles follow each other with such rapidity that air cannot enter by the lateral orifices; but so soon as we diminish the flow, the stream becomes narrower, consequently its velocity below the orifice of C is less than that above it, because the adhesion of the glass of A causes the stream, although of less diameter, to completely fill it; air is now able to enter by the lateral orifices, and if water were not constantly flowing from C the whole column would of course fall; but as soon as a small amount of air has entered, the flowing water closes the orifices, so that we get periods during which air enters, alternating with periods during which the orifices are closed.

The musical notes are produced by the periodic breaking of the continuity of the column of water by air, the regular entry of which causes regular pulses to be given to the air, which, when they follow each other with a certain rapidity, produce a musical note. A slight movement of the tube often stops the production of the musical note, and the air then enters irregularly, and escapes from the lower orifice of the tube in jerks; but by pitching the voice to the note previously produced, the air enters regularly, the column assumes the peculiar banded appearance, the musical note is produced, and air issues from the lower orifice of the tube in a regular stream.

When the stream was rapid, the air which entered did not completely break its continuity, but passed down the axis of the tube in small bubbles (A, fig. 3). No sound was apparent when this occurred. On diminishing the flow the banded appearance set in, and when viewed by means of a moving mirror, was seen to be produced by alternate discs of air and water (B, fig. 3), the former being doubly convex, and the latter doubly concave, as we should expect. When the flow was further dimin-

Fig. 3.





ished; the discs were of nearly equal size (C, fig. 3); and when the flow was very much diminished (that is to say during the production of the most base notes), cylinders of air alternated with discs of water (D, fig. 3).

The air was collected in a half-litre flask, inverted over the lower orifice of A, fig. 2, the exact time necessary to fill it being noted, and the amount of air passing through the tube in a second calculated therefrom.

When the flow of water was so small that impulses not following each other with sufficient rapidity to produce a musical note were heard, the eye could follow the air-cylinders in their passage down the tube, and by attaching a small divided scale to the side of the tube A, fig. 2, and simultaneously checking the flow and closing the orifices, the number of air-discs in an inch could be counted; and thus, the amount of air and of water passing through the tube per second, the capacity of the tube, and the number of air-discs in it at one time being known, the number of impulses produced per second could be roughly calculated.

When the flow had attained a certain degree of quickness, the air-discs coalesced into larger ones so rapidly that the number in an inch could not be counted.

All the notes were base; and the only variation capable of being produced was from a base note to distinct impulses incompetent from their slowness to produce a note.

The following Tables may serve as examples of some of the results obtained: it is obvious that the number of impulses per second could not be determined with any great accuracy:—

I. An 18-inch vertical tube, A, fig. 2. Caoutchouc with four orifices, B, fig. 2.

Water passing through the tube per second.	Air passing through the tube per second.	Number of impulses per second.
cub. centims.	cub. centims.	
25	Air did not enter.	} Could not be determined, on account of the too rapid coalescence of the air discs.
21·739	·333	
17·241	2·403	
15·151	2·994	
11·111	4·761	
10·638	5·263	
5·263 Highest note produced.	4·761	
3·906 Lower note than preceding.	4·346	
3·571 " " "	4·166	
2·941 " " "	3·787	
1·872 Most base note.	3·787	38·2
1·766 Distinct impulses.	3·703	30·8
·636 " "	2·564	14·5

II. A 12-inch vertical tube. Caoutchouc with four orifices.

Water passing through the tube per second.	Air passing through the tube per second.	Number of impulses per second.
cub. centims.	cub. centims.	
25	Air did not enter.	} Could not be deter- mined on account of the too rapid coalescence of the air-discs.
20	·811	
13·888	2·617	
13·157	2·994	
10·638	4·000	
4·504 Highest note produced.	4·032	59·8
3·731 Lower note than preceding.	3·496	50·6
2·941 " " "	3·759	46·9
2·439 " " "	3·333	37·5
2·325 { Most base note, deeper than any note produced in Experiment I. }	3·086	35·0
1·018 Distinct impulses.	2·293	19·8

IV. On Laplace's Correction for the Velocity of Sound.

By JOHN TYNDALL, F.R.S.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

IN the last November Number of the *Philosophical Magazine* I published a brief "Note on Laplace's Correction for the Theoretic Velocity of Sound," the main point of which was to show that the extraordinary radiant and absorbent power which recent experiments have proved to be possessed by various gases and vapours, would, in all probability, affect the correction to be applied to the velocity of sound through such gases and vapours. The portion of the note which refers to this point, resting as it does on sure experiments, will, I think, always retain its force and pertinence: this portion terminates with the words "nor could a correct ratio be deduced from the velocity of sound in the medium."

The remaining portion of the note I would ask permission to cancel for the present. I have recently made some experiments with a view to testing the possible influence of radiation; and if the continuance of these should lead to any definite result, they shall in due time be communicated to the *Philosophical Magazine*.

I am, Gentlemen,

Your obedient Servant,

JOHN TYNDALL.

V. *Analytical Theorem relating to the four Conics inscribed in the same Conic and passing through the same three Points.* By A. CAYLEY, Esq.\*

IMAGINE the four conics determined, and, selecting at pleasure any three of them, let their chords of contact with the given conic be taken for the axes of coordinates, or lines  $x=0$ ,  $y=0$ ,  $z=0$ ; and then taking for the equation of the given conic

$$U = (a, b, c, f, g, h)(x, y, z)^2 = 0,$$

the equations of the selected three conics must be of the form  $U + lx^2 = 0$ ,  $U + my^2 = 0$ ,  $U + nz^2 = 0$ , where  $l, m, n$  are to be determined in such manner that these conics may have three common points; the resulting values of  $l, m, n$ , and of the coordinates of the three common points, that is, the three given points, will of course be functions of the coefficients  $(a, b, c, f, g, h)$ ; and the equation of the fourth conic will be of the form

$$U + (ix + jy + kz)^2 = 0.$$

There is no difficulty in carrying out the investigation. It is found that the coordinates of the given points must be taken to be

$$(-f, g, h); \quad (f, -g, h); \quad (f, g, -h)$$

respectively, and that, writing as usual

$$K = abc - af^2 - bg^2 - ch^2 + 2fgh,$$

the equations of the four conics are

$$U + (K - abc) \frac{x^2}{f^2} = 0,$$

$$U + (K - abc) \frac{y^2}{g^2} = 0,$$

$$U + (K - abc) \frac{z^2}{h^2} = 0,$$

$$U + (K - abc) \left( \frac{x}{f} + \frac{y}{g} + \frac{z}{h} \right)^2 = 0.$$

It is in fact easy to verify directly that each of these conics passes through the three given points; but the equations may also be exhibited in the form proper for putting this in evidence. Putting, for shortness,

$$X = \frac{y}{g} + \frac{z}{h}, \quad Y = \frac{z}{h} + \frac{x}{f}, \quad Z = \frac{x}{f} + \frac{y}{g},$$

the equations of the sides of the triangle formed by the given points are  $X=0$ ,  $Y=0$ ,  $Z=0$ , and the foregoing equations of

\* Communicated by the Author.



the four conics may be expressed in the form

$$\begin{aligned} (-bg^2 - ch^2 + 2fgh)YZ + & \quad bg^2 \cdot ZX + & \quad ch^2 \cdot XY = 0, \\ af^2 \cdot YZ + (-ch^2 - af^2 + 2fgh)ZX + & & \quad ch^2 \cdot XY = 0, \\ af^2 \cdot YZ + & \quad bg^2 \cdot ZX + (-af^2 - bg^2 + 2fgh)XY = 0, \\ (-bg^2 - ch^2 + 2fgh)YZ + (-ch^2 - af^2 + 2fgh)ZX + (-af^2 - bg^2 + 2fgh)XY = 0 \end{aligned}$$

which is the required form.

Cambridge, November 28, 1863.

VI. *Analytical Theorem relating to the Sections of a Quadric Surface.* By A. CAYLEY, Esq.\*

THE four sections  $x=0$ ,  $y=0$ ,  $z=0$ ,  $w=0$  of the quadric surface

$$ax^2 + by^2 + 6xy \sqrt{ab} - cz^2 - dw^2 = 0$$

are each of them touched by each of the four sections

$$x \sqrt{2a} + y \sqrt{2b} \pm z \sqrt{c} \pm w \sqrt{d} = 0;$$

where it is to be noticed that the radicals  $\sqrt{2a}$ ,  $\sqrt{2b}$  are such that their product is  $=2\sqrt{ab}$  if  $\sqrt{ab}$  be the radical contained in the equation of the surface. There is of course no loss of generality in attributing a definite sign to the radical  $\sqrt{2a}$ ; but upon this being done, the sign of the radical  $\sqrt{2b}$  is determined, whereas the signs of  $\sqrt{c}$  and  $\sqrt{d}$  are severally arbitrary. We may if we please write the equation of any one of the last-mentioned sections in the form

$$x \sqrt{2a} + y \sqrt{2b} + z \sqrt{c} + w \sqrt{d} = 0,$$

it being understood that the radicals  $\sqrt{2a}$ ,  $\sqrt{2b}$  have each a determinate sign, but that the signs of  $\sqrt{c}$  and  $\sqrt{d}$  are each of them arbitrary.

To prove the theorem, it is enough to show (1) that the sections  $x=0$ ,  $x \sqrt{2a} + y \sqrt{2b} + z \sqrt{c} + w \sqrt{d} = 0$ ; (2) that the sections  $z=0$ ,  $x \sqrt{2a} + y \sqrt{2b} + w \sqrt{d} = 0$ , touch each other.

1. The sections  $x=0$ ,  $x \sqrt{2a} + y \sqrt{2b} + z \sqrt{c} + w \sqrt{d} = 0$  of the surface  $ax^2 + by^2 + 6xy \sqrt{ab} - cz^2 - dw^2 = 0$  will touch each other if, combining together the equations

$$x=0, \quad y \sqrt{2b} + z \sqrt{c} + w \sqrt{d} = 0, \quad by^2 - cz^2 - dw^2 = 0,$$

these give a twofold value (pair of equal values) for the ratios

\* Communicated by the Author.

$y : z : w$ . And we in fact have

$$\begin{aligned} by^2 - cz^2 - dw^2 &= by^2 - cz^2 - (y \sqrt{2b} + z \sqrt{c})^2 \\ &= -by^2 - 2cz^2 - 2yz \sqrt{2bc} \\ &= -(y \sqrt{b} + z \sqrt{2c})^2; \end{aligned}$$

and the right-hand side being a perfect square, the condition of contact is satisfied.

2. In like manner we have the system

$z=0$ ,  $x \sqrt{2a} + y \sqrt{2b} + w \sqrt{d}=0$ ,  $ax^2 + by^2 + 6xy \sqrt{ab} - dw^2=0$ , which gives

$$\begin{aligned} ax^2 + by^2 + 6xy \sqrt{ab} - dw^2 \\ &= ax^2 + by^2 + 6xy \sqrt{ab} - (x \sqrt{2a} + y \sqrt{2b})^2 \\ &= -ax^2 - by^2 + 2xy \sqrt{ab} \\ &= -(x \sqrt{a} - y \sqrt{b})^2; \end{aligned}$$

so that, the right-hand side being a perfect square, the condition of contact is satisfied.

Cambridge, November 28, 1863.

## VII. On the Absorption of Light. By A. WÜLLNER\*.

**I**N a congratulatory paper, written on the occasion of Professor Gerling's fiftieth-year-Doctor Jubilee in Marburg, I communicated some experiments on the spectra of the incandescent vapours of iodine, bromine, and hyponitric acid. I was led to these experiments by the notice of Mr. Thomson, in the *Annales de Chimie et de Phys.* 3 sér. vol. lxii. p. 191, in which he communicates an earlier idea of Mr. Stokes, which includes a mechanical explanation of the beautiful observations of M. Kirchhoff on the absorption of light in flames, and of the absorption of light in general. Mr. Stokes's reasoning is as follows:—The emission of light has its origin in a periodic motion of the molecules of the body, which is communicated to the æther surrounding them; the emission of a definite quality of light proves hence that the molecules of the flame are in a definite periodic motion. Incandescent soda vapour, which emits yellow light corresponding to the dark line D, will hence have such a periodic vibratory motion, its particles will have a duration of oscillation corresponding to that of the æther for yellow light.

For just the same reason sodium vapour surrounding a source of light must have a tendency to retain in itself light of the

\* Translated from Poggendorff's *Annalen*, Oct. 1863.

same quality, inasmuch as the vibrations of this kind of light must be enfeebled and diminish in velocity, because at each vibration the æther-molecules clash with the adjacent sodium-molecules vibrating in the same phase.

It readily suggested itself, with reference to the theory of absorption developed by Mr. Stokes\* in an earlier paper, to extend this idea to all phenomena of absorption—to see the absorption of light based on the fact that the vibratory molecules of æther communicate their velocity to the molecules of the absorbing body, which perhaps are only for the first time caused by the light to vibrate in phases appropriate to their molecular structure. It followed further therefrom, that if any body or gas at ordinary temperatures exhibited a special power of absorption for certain kinds of light, then the body in question, in virtue of its molecular structure, is especially fitted for the corresponding oscillations. If therefore the body in question does not change its nature at high temperatures, it must, if heated to that temperature at which it can emit light, send out especially that light which it absorbs at lower temperatures.

To prove this theorem, and therewith to find eventually a proof of the accuracy of the new theory of absorption, I endeavoured last year to establish a comparison of the absorption spectra of iodine, bromine, and hyponitric acid, with the spectra of the flames in which these vapours are incandescent. To obtain flames which only emit light arising from these vapours, I passed the vapours into the flame of hydrogen. To bring iodine, for example, into the flame, I passed hydrogen from a spacious gas-holder into a tube containing iodine vapour, the glass tube ending in a tolerably wide point, directed upwards, which was just in front of the slit of the spectrum apparatus. In the glass tube was a plug of asbestos, to prevent solid particles being carried into the flame. Further, the front end of the tube was laid in a sand-bath, and thus kept at a temperature at which iodine vapour does not condense. The iodine was placed at that end of the tube near which the hydrogen entered, and was heated with a spirit-lamp. The hydrogen thus became completely mixed with iodine vapour, which became incandescent in the flame when the gas at the point was lighted.

I operated similarly to bring vapour of bromine or of hyponitric acid into the flame.

I succeeded then in proving that the conclusions of the absorption theory were confirmed, in so far that those parts of the spectra of the flames were the brightest in which most dark lines appeared when daylight passed through the vapours. For example, in the absorption-spectrum of iodine, dark lines predo-

\* His investigation on Fluorescence.



minate in red, yellow, and green. Blue and violet appear enfeebled, it is true, especially if the iodine vapours were very dense; but bands could not be detected in this part of the spectrum. Corresponding to this, only red, yellow, and green showed themselves in the spectrum of iodine, and scarcely a trace of blue and violet. A similar agreement was observed between the spectra of bromine and hyponitric acid vapours, and the absorption spectra of these vapours.

With iodine vapour it was possible to recognize bright and dark bands in the spectrum of the flame, but not to furnish the proof that the bright bands of the flame corresponded to the dark lines of the absorption spectrum and inversely, since in both spectra the position of the bands could not be ascertained by my apparatus.

In the other flames no bands could be observed, since their light was too feeble to bear the necessary contraction of the slit.

The observation of the iodine spectrum acquired for me an additional interest after having had, in Bonn a few weeks ago, the opportunity of witnessing the beautiful observations of M. Plücker on the spectra of the gases, which that gentleman had the goodness to show me. When I saw the nitrogen spectrum, designated "the first" by M. Plücker, I at once recognized that the iodine spectrum which I had seen was not the second iodine spectrum previously observed by M. Plücker, but a first one, which M. Plücker since his discovery of the double spectra had predicted, and in the observation of which he expected the same proof for the new theory\*.

\* In reference to the observation of M. Plücker on the first and second spectra of incandescent gases, I communicate, with M. Plücker's permission, the contents of a letter which he sent me in answer to an epistolary communication of the following observations. M. Plücker writes,—"In the well-known historical notice of Dr. Miller in London, 'On Spectrum Analysis,' the objection is made to my theory of the absorption of light that the iodine spectrum, which I had determined in 1859, stands in no relation to that obtained if sunlight (or, better, light which for itself gives a continuous spectrum) is passed through iodine vapour. The spectrum of iodine consists of bright lines on a dark background; the absorption spectrum, of about fifty compartments which are peculiarly shaded by dark lines. Spectra of the first kind I have subsequently designated as 'second spectra'; spectra like the one in question, 'first spectra,' to which the nitrogen spectrum, described in 1858, also belongs.

"From the investigations made in the physical laboratory of this place in August last year, by Professor Hittorf and myself in common, it was found that certain bodies, which at present it would be rash to say were not simple (nitrogen, sulphur, &c.), give, according to the temperature, two absolutely different spectra—a first one that corresponds to the lower, and a second one that corresponds to the higher temperature. The transition from the one to the other is discontinuous. In consequence of this novel and unforeseen fact, Miller's objection appeared to me under a new point of

I lately received from MM. G. and J. Mertz in Munich a prism of flint-glass of very great dispersive power. This led me to resume the experiments of the previous year, since I could hope by its means to determine at least the position of the previously observed dark lines in the iodine spectrum.

With this view the prism was fitted in one of Steinheil's spectrum apparatus, which was provided with a scale in the ordinary manner, in order to be able to fix by its divisions the light or dark lines in the spectrum seen through the apparatus.

And first the position of the absorption bands in the spectrum of daylight which had passed through iodine vapour was determined. The bands were seen about as far as the Fraunhofer's line F; the most refrangible part of the spectrum was certainly enfeebled, but bands I could not detect in it.

So long as the iodine vapours were not too dense, the absorption bands appeared in my prism, between the Fraunhofer's lines C to almost F, as almost equally distant black bands, so that the bright intervening space was of almost the same breadth as the black bands. If the iodine vapours became denser and denser, the character of the absorption spectrum in the red and yellow, as far as green, did not change; the individual dark bands were only darker and a little broader. At the same time the green, as the iodine vapour became denser, appeared to become covered with dark red, which gradually became denser, and finally completely extinguished the green; but as long as the bands were visible, they did not appear to have become broader than the others.

It must, however, be mentioned, in reference to the absorption in the green part of the spectrum, that even from the beginning the bright spaces between the absorption bands appeared darker, so that it was difficult to fix sharply the individual dark lines.

The following small Table contains the position of a number of bands determined on the scale of the apparatus—that is, of those which were visible from near C to about  $D \frac{1}{2}$  E. To obtain a representation of this, it may be mentioned that the scale is divided into 250 parts, of which the spectrum covered about 200. The spectrum appears to lie upon the scale, whose divisions are feebly illuminated; but the dark bands can be seen above, between, and below the divisions, so that their position can be determined with great certainty.

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view. On the basis of the theory of absorption in question, I predicted, together with the second iodine spectrum, one corresponding to a lower degree of heating as negative image of the absorption spectrum; and inversely, I expected in the realization of this prediction a beautiful confirmation of the theory of absorption."

On this scale the Fraunhofer's lines

C lay between the divisions 67 and 68,  
D        „        „        86 „ 87.

The dark lines of the absorption spectrum were upon and between the divisions

72	90—91	
73—74	92—93	
75—76	94—95	
77—78	96	
79—80	98	
81	100	Limit of the green ; the follow-
83	101—102	ing dark lines are no longer
85	103—104	visible with a certain thickness
86—87 Fraunh. D	105—106	of iodine vapour.
88—89	107—108	
	109—110	

In order to compare now the spectrum of the iodine flame with the absorption spectrum, I proceeded essentially as in the previous case: the hydrogen flame loaded with incandescent iodine vapour was brought in front of the slit of the spectrum apparatus. To protect the apparatus from the action of iodine vapours, and to ensure the regular combustion of the flame, it was surrounded by a cylindrical lamp-glass.

The hydrogen flame, in which there is not much iodine vapour, is only feebly luminous, and that with a green light; it is more luminous, and that with reddish-yellow light surrounded by a feebly luminous border, if the hydrogen gas is strongly charged with iodine vapour so that a strong current of red vapours issues from the flame.

If care is taken that the brightest part of the flame is in front of the slit of the spectrum apparatus, a glance into the telescope is sufficient to show the surprising similarity between the character of the flame spectrum and of that produced by daylight passed through iodine vapour.

From about the place corresponding to Fraunhofer's line C, the flame spectrum, like the absorption spectrum, appears composed of alternating bright and dark lines arranged like a barred grating, most perceptibly till the beginning of the green.

The green appeared much more continuous; the dark bands in it could scarcely be distinguished. The latter were much more distinctly seen if the flame, with a smaller quantity of iodine, was feebler, and, as before said, had a greenish light; the agreement in the character also of this part of both spectra was then very



evident. But even then the dark bands could not be fixed and their position in the scale determined, since the phenomenon was altogether too indistinct, and since it was almost impossible to keep the flame longer in this position, on account of the difficulty, with a spirit-lamp, of regulating the temperature of the iodine-tube so that not too much and not too little iodine may pass into the flame\*.

It was also difficult to determine the more distinct bands in the red and yellow, because a good spectrum could only be obtained for a short time; yet in a number of experiments their position could be determined from about C to something beyond the part of the spectrum corresponding to D. The following small Table contains the *dark* bands of the flame spectrum whose position could be determined with certainty.

The dark bands in the flame spectrum lay upon and between the divisions of the scale

70—71	84
72—73	86
74—75	88
76—77	91—92
78	93—94
82	95—96

The links between the numbers 78 and 82, and between 88 and 91, do not mean that there are no dark bands there; they depended on the fact that the last observations, which would have given me the bands between 80 and 90, had to be interrupted owing to an unfortunate accident.

If the dark bands in the absorption spectrum of daylight which has passed through iodine vapour be compared with those in the flame spectrum, it is seen, in the most surprising manner, that the dark bands in the flame spectrum are where the absorption spectrum has bright lines, and inversely; for the dark bands lay upon and between the divisions—

\* As M. Plücker tells me, in the letter mentioned above, he has in just such an iodine flame recognized the iodine spectrum corresponding to the absorption spectrum. He writes:—

“From the beginning of my investigations, when with the aid of an induction current I obtained the spectrum of an enclosed rarefied gas, I designated this phenomenon as one solely depending on the heating by the current, and proposed to myself to make the gas incandescent in another way. The older experiments, especially upon iodine, were imperfect; new experiments were already commenced at the time of your visit; the arrangements which M. Ste.-Claire Deville uses for producing high temperatures were in my hands. Since your departure I have, as a preliminary experiment, prepared the first spectrum of iodine in the manner in which you proceeded, and which we discussed.”



In the absorption spectrum.	In the flame spectrum.
72	70—71
73—74	72—73
75—76	74—75
77—78	76—77
79—80	78
81	82
83	84
85	86
86—87 Fraunh. D	88
88—89	
90—91	91—92
92—93	93—94
94—95	95—96
96	

The conclusions of the absorption theory are thus seen to be confirmed in the most beautiful manner by the spectrum of incandescent iodine gas: incandescent iodine just emits that light which the violet vapours of iodine absorb.

I did not succeed in obtaining this agreement in the case of bromine vapour; for the light of its flame was too feeble to give its spectrum in a state of purity.

VIII. *On deducing the Parallax of Mars, and thence that of the Sun; from the former's Geocentric Velocity when in opposition, and especially near the Node.* By S. M. DRACH, F.R.A.S.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

WITH the above title I presented in 1843 a paper to the Royal Astronomical Society (*vide* Monthly Notices, vol. ix. Jan. 1844); and as a similar mode of finding the true value of the sun's parallax has lately and successfully been practised, I beg to communicate, through your Journal, the complete paper, only a short extract of which was at the time inserted in the 'Monthly Notices' of the Royal Astronomical Society.

"The transits of Mercury and Venus have hitherto been deemed the only accurate means of ascertaining the small quantity of solar parallax, by observing the time employed by those planets in traversing the sun's disc. The effect of parallax, though hardly measurable with exactness in space, was here so much magnified that (*vide* O. Gregory's 'Astronomy,' p. 385) the observed differences of duration at Otaheite and Wardhuus amounted to 23<sup>m</sup> 10<sup>s</sup>, which considerable interval, properly applied to the

velocity of Venus in her orbit, led to the exact determination of the sun's parallax.

"Now the counterpart of the above is the simultaneous observation at different parts of the earth's surface, of the time occupied by a superior planet, when near opposition and near the node, in passing through a certain interval of space, say about half a degree (the sun's diameter); but as this happens at night, comparison stars are to be used, and the assumed interval to be nearly equivalent to their distance. Thus, *ex. gr.*, if the nearest planet (Mars) be the object observed, and at Greenwich  $x$  minutes are occupied by it in describing an arc which it requires only  $y$  minutes to describe at the Cape of Good Hope, then will the difference  $x-y$  properly applied give the parallax of Mars, and hence that of the Sun.

"It is, in short, the observation for a like end which an observer at Venus would have made of the Earth at the former's transit. That such observations on Mars may differ ten or twelve minutes in time, the following investigation will possibly show. According to Vince's 'Astronomy,' part 3, we had in 1840—

1840.	Venus.		Mars.	
	Planet.	Earth.	Planet.	Earth.
Ascending node .....	75° 12' 48"	° ' "	48° 19' 24"	° ' "
Aphelion.....	309 08 06	100 10 39	153 07 57	100 10 39
Min. long. hypoth.....	75 51 00	.....	38 10 00	.....
True ecl. long. ....	75 13 04	75 13 04	38 17 36	38 17 36
Do. next day .....	76 40 50	76 14 03	38 50 38	39 17 41
Log. curt. dist. ....	9·857570	9·993322	0·168847	9·995103
Do. next day .....	9·857508	9·993269	0·169232	9·995304
Geoc. diff. long. ....	1 25 52	.....	1 31 03	.....
Hor. par. from ☉ .....	12''·28	.....	5''·96	.....
Described with geoc. } motion in.....	204 <sup>s</sup> ·84	.....	94 <sup>s</sup> ·26	.....
Hor. par. from ☿ .....	33''·30	.....	18''·12	.....

"Maskelyne's formula, in O. Gregory's 'Astronomy,' p. 383, as applied to the transit of Venus in 1769 for the parallax-caused differences in times of ingress and egress, is representable by  $204^s\cdot84 (A \pm B)$ ; and for the counterpart observations of Mars when passing through the same arc in the opposite part of the heavens, the above formula is modified to  $94^s\cdot26 (A \pm \cdot4879B)$ . The preceding Table was calculated for the mean parallax  $8''\cdot80$ .

$204\cdot84 A \propto \text{parallax} = 1417\cdot05$  } for  $8''\cdot80$ , {  $1412\cdot22$  } for  
 $204\cdot84 B \propto (\text{parallax})^2 = -10\cdot10$  } and {  $-10\cdot05$  }  $8''\cdot83$

Therefore  $94^s\cdot26 A = 649^s\cdot81$ , and  $94\cdot26 \times \cdot4879B = -2^s\cdot25$  for parallax  $8''\cdot80$ . Hence  $649^s\cdot81 - 2^s\cdot25 = 647^s\cdot56 = 10^m 47^s\cdot56$

is the difference in time for Mars traversing the same celestial arc at Otaheite and Wardhuus, an arc less than  $0^{\circ} 32'$ .

"Several advantages are presented by this mode of ascertaining parallax, if feasible. First, the inclination of the orbit of Venus is  $3^{\circ} 23' 35''$ ; whilst that of Mars is only  $1^{\circ} 51' 04''$ , or three-fifths of the other. Secondly, the observations can be made at all oppositions with comparison stars, and the reductions to the simple nodal passage easily performed. Thirdly, whilst a great number of positions are necessary for the very rare transit of an inferior planet, to obviate the possibility of cloudy weather obscuring the solar disc, and where only travellers' instruments can be used; these observations may be conducted simultaneously by professional astronomers, with first-rate instruments, at a few of the principal observatories, *e.g.* Greenwich, the Cape, Paramatta, Dorpat, and Madras.

"S. M. DRACH."

"November 30, 1843."

P.S. I think I may fairly claim to having drawn attention to this important method *twenty years* ago. As the *travelling opinion* of a most eminent foreign observer was rather against the measuring of a planet from a star with equal accuracy to the observing the critical moments of a transit over the sun's disc, I bowed to the said decision, although it struck me as singular that the kind of observations by which we fix the place of a new comet should not be applicable for the above purpose. S. M. D.

December 14, 1863.

## IX. On the Dynamics of the Galvanic Battery.

By JAMES NAPIER, Esq.

To William Francis, Esq.

66 Miller Street, Glasgow,  
December 15, 1863.

DEAR SIR,

READING over the interesting paper of Professor Williamson "On the Dynamics of the Galvanic Battery," I observe that he, in describing the electric current passing through a solution of a salt, has assumed that the current of electricity, in decomposing the salt, of necessity causes a mutual transfer, or passage, of the elements of the salt to the different metallic electrodes or battery plates: thus, in the decomposition of hydrochloric acid the chlorine passes to the zinc, and the hydrogen to the copper. Referring to the diagram, page 460 of the December Number of this Magazine, he says,—

"In the annexed diagram, Z represents the zinc plate, C represents the copper plate, and between them we have the chain of

molecules of hydrochloric acid arranged in polar order from zinc to copper. If we remove from our diagram the chlorine from one end and the hydrogen from the other, we have a chain of molecules in the order in which they are kept after the first action, an order which we may call antipolar. If matters remained in this order, there could be no further combination of zinc with chlorine, no further liberation of hydrogen on the copper. We see that there can be no continuous current without a motion of the atoms: the atoms of chlorine must come in contact with the zinc to combine with it, and the atoms of hydrogen in contact with the copper to escape from it, and the original polar arrangement must re-establish itself throughout the whole liquid from the zinc to the copper before the next decomposition ensues. It would be unnecessary to discuss at present the particular manner in which this atomic motion takes place in the conducting liquid; but there is no doubt of the fact that each atom of chlorine has to take the hydrogen from another atom of chlorine after each decomposition, and then to get over so as to touch the positive plate, while the hydrogen gets next to the negative plate. These atomic motions have been long since shown to take place in the conducting liquid."

In the *Philosophical Magazine*, vol. xxv. (1844), there is a paper by the late Professor Daniell and Dr. Miller, in which is announced the fact that a mutual transfer of the elements of a decomposing salt by an electric current does not take place: and my own experiments, an account of some of which was read before the Chemical Society several years ago, made with a variety of salts in solution, both on a large and small scale, also show that the acid element of a salt is alone transferred by an electric current.

Suppose a vessel divided by a porous diaphragm have dissolved in each division an equal quantity of a salt (say sulphate of copper), and into each of the divisions is placed a plate of metal (say copper) attached to the poles of a battery, which completes the circuit. Now by Professor William's assumed theory, there would be a mutual transfer of the acid and copper between the two divisions, so that at any time, if the operation or current of electricity was stopped, the solutions in the two divisions would be the same as when the experiment began; but the facts of the case are not so. The copper in solution in the division attached to the zinc plate of the battery will be deposited as metal on the copper plate in that division, while the acid element will be transferred to the other division; but the copper in that other division will not come into the division in the opposite direction of the acid; so that at the end, the one division will have neither copper nor acid in solution, and the other



division (that connected with the copper of the battery) will have double the quantity of sulphate of copper in solution that it had at the commencement.

There is no doubt but the polar theory with the mutual transfer of elements is simple and beautiful, and has become popular ; but a theory, to be correct, must adapt itself to facts. I have seen nothing yet to explain away the facts pointed out by the late Professor Daniell, Dr. Miller, and myself, and take this opportunity to draw the attention of Professor Williamson and others interested in such inquiries to the subject, in hope of getting further information.

I am, yours truly,

JAMES NAPIER.

X. *On the Gas Battery of Mr. Grove ; and its Theory.* By T. A. MALONE, late Director of the Laboratory, London Institution\*.

HAVING had, during the past summer, occasion to review minutely the theory of the voltaic pile, I was, by the nature of my work, led to go somewhat fully into an examination of recorded facts and observations in regard to the whole subject—and this by various writers, chemical and electrical, old and modern. The results of my inquiry and attendant experiments have compelled me to lay down different interpretations of the origin of the power and nature of the phenomena of the pile from those usually adopted by the advocates of the chemical and contact theories, or of the theory of Davy, which makes both contact-electrical and chemical action essential to the origin and continuance of the current. Nevertheless I may in passing observe that, with an important qualification as to the origin of the electric agency, I incline to a revival of the latter theory. My reasons for doing so will be apparent a little later. I know, however, that with respect to the theory of the voltaic pile, speaking generally, I dare not at present express fully my scientific convictions, since the facts on which I rely are in part secondhand, and, although the work of eminent men of former days, may be thought by some to need revision on account of our extended knowledge. At the same time it is right to say that I have no doubt as to the consequences of such revision. But with regard to the gas battery considered by itself, I have no such cause for hesitation ; there everything of a chemical and electrical nature essential to the inquiry is apparently so open and under control and ready observation, that I do not see

\* Communicated by the Author.

that I need, as to my facts and views on this subject, shrink from an appeal to the judgments of the competent readers of this eminently philosophical publication.

The gas battery of Mr. Grove, as most readers are aware, consists of a series of cells, each containing two tubes of glass sealed at one end, and in which are inserted by fusion long narrow platinum plates and attaching wires, the plates reaching a little below the bottom of the tubes. These tubes are partly immersed by means of a ground collar into a square three-necked Woulfe's bottle, the centre hole between the tube-holes being stoppered. The tubes are usually two-thirds filled, the one with hydrogen two volumes, the other with oxygen one volume, the bottle and remaining part of the tube holding the usual dilute sulphuric acid to complete the circuit. The platinum is, in order to promote contact with the respective gases and the liquid, covered with platinum-black; metallic contact between the tubes or to the galvanometer is made by mercury cups attached to the platinum wires. I have been thus particular in describing the form of cell, because my case rests upon the results obtained by a pair which I have constructed and charged in a similar manner in all apparent essentials, with only one important difference (as the fact turns out), namely, omission of the platinum-black. In my pair I simply use plain clean platinum, first burnished with agate and then further cleansed with tripoli, ammonia, and spirits of wine; so that it is chemically and electrically clean, as experiment easily shows. Thus far premised, and all being arranged, I proceed to experiment with each instrument. With Mr. Grove's pair, which I will call A, I get of course a powerful deflection with an ordinary good galvanometer; with the plain platinum pair B, none. The A-tubes left in metallic contact for some time show a tolerably rapid disappearance of the gases. The B-tubes left in contact for the same time, no sensible disappearance. But, more than this, the A-tubes left without voltaic metallic contact show a rather rapid rise of fluid, especially in the hydrogen one. The B-tubes in a parallel experiment, in which the time is not too long, show no alteration of level, but after several days a little hydrogen has disappeared: this I attribute to the difficulty of burnishing the edges of the foil used.

The whole of the facts needed for our purpose are now before us. There is, as might have been foreseen, evident chemical union of free gaseous particles, obtained by diffusion, in the tube containing platinum-black, and none sensibly in the plain-platinum tube; and since in the one case there is a current following metallic contact, and in the other none, it follows, I humbly submit, that antecedent static electric action, produced here, I admit, by chemical action (but of direct combination, and not of indirect

with simultaneous decomposition), is necessary to produce true voltaic phenomena. Contact in the wire, or disruptive discharge in the air, or convection in an external decomposition-cell, restores the disturbed equilibrium of the chain. The fluid below, being part of the chain, suffers decomposition as a consequence of the discharge above, and, as Davy would allow, the now liberated gases react directly upon the free gases in the tube; and so a constant supply of active material is furnished for the production of fresh electricity, and hence the appearance of a current and of circulation. The antecedent chemical action and statical spark is now no longer necessary, unless contact be broken, when all reverts to the original state, and the diffused and dissolved gases are again in requisition. Fresh contact, by allowing discharge of static electricity, will again renew the voltaic or galvanic phenomena.

Such I believe to be a true and philosophical account of the facts and consequences concerned in this remarkable instrument; and I venture to think it is one that will apply also to the water and salt-and-water excited battery, if not to the acid battery itself. Thus the rival theories find their place and application, but not in the order or manner supposed by their first and illustrious enunciators, Volta, Wollaston, and Davy. If truth is found in these facts and views, chemists and electricians will readily see that such topics as passive iron, rusting of metals, and decay, and even general chemical action itself, may yet receive further elucidation from viewing them in the manner here attempted in the case of the gas battery. I may at once mention that the anomaly of free hydrogen being found with the nitrogen in some of Mr. Grove's eudiometric experiments becomes perfectly intelligible and regular by the considerations here evolved.

I am indebted to Mr. Gassiot, whose known kindness needs no comment, for the use of the Grove gas pair, and also to Dr. Wrigley for permission to carry on my experiments in his laboratory, aided by his instruments.

The Grammar School, High Street,  
Clapham, December 24, 1863.

XI. *On the History of the Principle of the Conservation of Energy.*  
By A. COLDING, Member of the Royal Society of Denmark.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

Copenhagen, November 1863.

AS the libraries of Copenhagen do not contain the Philosophical Magazine and Journal of Science, I have hitherto got all my information about various memoirs published in the



Philosophical Magazine from other journals, and especially from Poggendorff's *Annalen der Physik und Chemie*. Since, however, many beautiful investigations constantly appeared in the Philosophical Magazine, I was at last induced to become myself a subscriber to your celebrated Journal, and a few weeks ago I received the Numbers that have been printed from the beginning of this year till the first of October.

On looking over the contents I found several papers and memoirs of great interest to me; but those of course are most particularly interesting which treat of the new principle in relation to the forces of nature, which in Danish is called "The principle of the imperishableness or perpetuity of Energy (*Princippet for Kræfternes Uforgængelighed*), the most important part of which is the Mechanical Theory of Heat; and the questions connected therewith that have lately been taken into consideration in your Journal, and especially the historic notice of the mechanical theory of heat by M. Verdet, in No. 170 of the Philosophical Magazine (S. 4. vol. xxv. p. 467), induce me to send you the following remarks, and to ask you to decide upon a question I shall afterwards have the honour of laying before you, the favourable decision of which will give me very great pleasure.

From the contents of the Numbers of the Philosophical Magazine which I have received (4th series, Nos. 165 to 175), I may suppose that you are aware that M. Verdet and M. Helmholtz think it right, according to what is known to them of my investigations, to place me next to M. Mayer in relation to the discovery of the new principle touching the forces of nature; but at the same time I learn that many of the readers of your celebrated Journal have only a slight notion of my investigations and memoirs on this subject, and that it may be presumed that most of them have scarcely any idea at all about them.

I regret very much that the Danish language is so little understood beyond the Scandinavian countries, as I learn from M. Verdet's historic notice is the case, although our Royal Society of Science regularly exchange their Transactions, in which my memoirs are printed, with most of the celebrated foreign societies; but as I think it would perhaps interest your readers to see what I have done, how I have done it, and at what time, and as it is of course a matter of importance to me to be understood, and to be placed in that position in relation to this discovery to which I have a claim, I hope you will allow me to give here a short outline of my investigations touching the new and important principle of energy.

The first idea I conceived on the relationship between the forces of nature was the following.

As the forces of nature are something spiritual and immaterial,



entities whereof we are cognizant only by their mastery over nature, these entities must of course be very superior to everything material in the world; and as it is obvious that it is through them only that the wisdom we perceive and admire in nature expresses itself, these powers must evidently be in relationship to the spiritual, immaterial, and intellectual power itself that guides nature in its progress; but if such is the case, it is consequently quite impossible to conceive of these forces as anything naturally mortal or perishable. Surely, therefore, the forces ought to be regarded as absolutely imperishable (*uforgængelige*).

It was in accordance with this idea that I twenty years ago presented to the Royal Society of Science, here in Copenhagen, a treatise in which I explained my idea that force is imperishable and immortal; and therefore, when and wherever force seems to vanish in performing certain mechanical, chemical, or other work, the force then merely undergoes a transformation and reappears in a new form, but of the original amount as an active force.

In the year 1843 this idea, which completely constitutes the new principle of the perpetuity of energy, was distinctly given by me, the idea itself having been clear to my own mind nearly four years before, when it arose at once in my mind by studying D'Alembert's celebrated and successful enunciation of the principle of active and lost forces; but of course the new principle was not as clear to me from the beginning as it was when I wrote my treatise in 1843. According to the view which led me to this principle, its future importance, in case it were really true, was perfectly clear to me from the first instant. But this made me very anxious not to publish it as a new law of nature until I should be able to give experimental proof of its truth; and scientific men to whom I explained my idea, and especially our celebrated Professor, H. C. Ørsted, agreed with me and advised me to be safe in this respect before I wrote; and it was for this reason that I departed from my original intention of explaining it to a meeting of Natural Philosophers held in Copenhagen in 1840.

In my first treatise, of 1843, the title of which is "Theses concerning Force" (*Nogle Sætninger om Kræfterne*), I therefore not only presented my idea to the Royal Society (of Copenhagen) as a thing that most likely would hereafter be found to be a general law of nature, but, after stating that the only trustworthy decision of the question was to be got from the experimental investigation of nature itself, I went on to call attention to several old experiments made previously to my time, the first of which was Du-long's celebrated discovery respecting the heat disengaged or absorbed during the compression or expansion of a great number

of different airs and gases, and I then showed how perfectly these experiments proved the truth of the said principle for bodies of that kind. After having obtained this general confirmation of the principle by elastic fluids, I tried to show that the experiments of M. Ørsted on the compressibility of (non-elastic) fluids and the heat disengaged thereby were in perfect accordance with my new principle; and after having shown this, the next step for me was to show that the heat disengaged on the compression of solid bodies also was proportionate to the quantity of mechanical energy expended; and from the experiments of Berthollet, Lagerhjelm, &c. on this subject, I found that here also everything spoke in favour of this principle.

After stating these facts, I proceeded to examine what had been done and discovered previously to that time with regard to the heat disengaged by friction, and I found here experiments on a large scale carried out by Rumford, Haldat, Morosi, &c., and I observed with great pleasure that all the results at which these experimenters arrived throughout satisfied the new principle. As these experiments were not made with a view to the determination of the relation between the quantity of heat developed and the mechanical energy expended, it is easy to understand that they were not sufficient to decide the question as to whether the heat disengaged is always proportionate to the mechanical energy lost by friction, and therefore I thought it quite necessary to make experiments myself on the quantity of heat developed by friction.

An apparatus was then constructed by which I was able to measure the quantity of mechanical power lost, and the quantity of heat disengaged by friction between

Brass and Brass,  
Brass and Zinc,  
Brass and Lead,  
Brass and Iron,  
Brass and Wood, and  
Brass and Woollen Cloth,

under different pressures and with different velocities; and the result of all these experiments, nearly 200 in number, was that the heat disengaged was always in proportion to the mechanical energy lost; and if we look more nearly at the figures given in my treatise, which I only presented as a preliminary one, it will be seen that, independently of the materials by which the friction and the heat arose, an amount of mechanical work equal to 350 kilogrammetres should be able to raise the temperature of 1 kilogramme of water  $1^{\circ}$  C., which is very near the proportion (365 to 1) that M. Mayer in 1842 supposed, but did not prove, to be right.

Having thus convinced myself that the new principle was confirmed, not only by all the experiments made before my time, but also by the results obtained from my own experiments, I had no hesitation in stating that there could be very little doubt that perfect series of experiments would show my thesis to be quite exact.

I closed my discussion by showing that the discovery of a *perpetuum mobile* would be possible if my principle was wrong.

After the completion of the above-mentioned preliminary and first memoir, of 1843, the Royal Society of Science furnished me with money for the construction of a more perfect apparatus than that which I had to employ in my first series of experiments; and with that apparatus I subsequently carefully repeated the experiments, and found the results to be not only more perfect than the first, but also quite in favour of my new principle.

At the same time I was very anxious to put the new principle in a mathematical form that would enable me to include it in the general formula of analytical mechanics by which all other physical problems are treated, in a way very different to that hitherto "incessantly" followed in the mechanical theory of heat by all other physicists since Carnot; and I have always been highly satisfied with the result to which these investigations led me, as it throws much light upon the well-known formula, and likewise connects together the different forces of nature in a simple, clear, and exact manner that will prove itself to be successful.

The results of these experiments and investigations were communicated to the Association of Natural Philosophers at a meeting held in Copenhagen in 1847, and are printed in their memoirs; they were afterwards laid before the Royal Society of Science, and are also printed in their Transactions, partly in 1848 and partly in 1850.

In the following year (1851) I presented to the Royal Society a memoir on the power of steam, which, based on the new principle, treats the problems of steam-engines in a way very different to that formerly followed by Pambour and afterwards by several other authors (according to the new principle) in the path of Sadi Carnot and M. Clapeyron, but which, as I think, is not less exact and instructive than the latter.

The last memoir I have transmitted to our Royal Society of Science on this subject contains "A Physical Investigation into the general relationship between the Intellectual Powers and the Forces of Nature."

In this memoir, contained in the Royal Society's Transactions for 1856, I endeavoured to show that the new principle of the



perpetuity of the forces of nature had made such progress in all branches of Natural Philosophy, that there was scarcely any doubt left that this principle would be found to comprise a true law for the whole of nature, and perhaps one of the grandest and most successful laws known to us.

The first conclusion I drew from the said principle was this : *As no power is lost in nature, so no power is able to grow up in nature except at the expense of some other power.* But if we accept the hypothesis of the celebrated Kant and Laplace, which supposes the world originally given by the Creator as an extremely subtile elastic fluid, rotating round an axis, and kept together by the mutual gravitation of the particles of the fluid, which they supposed to contain all the chemical elements, the new principle allows us to go back one step more than Kant and Laplace did, and to commence with the supposition that the original material substance had none of the qualities which characterize the chemical elements, except in so far as it was subject to the force of gravitation. Then, according to the said principle, it is evident that, without generating a quantity of energy (*vis viva*) equal to the work done by the force of gravity, no condensation of the fluid could take place; and as no loss of energy could occur, it may be admitted that the quantity of energy generated made its appearance in the form of electricity or chemical affinity, different in quality and quantity according to the situation of the matter in the universal globe, which, during the conversion of the quantity of energy thus generated into chemical affinity, may be supposed to have divided itself into a great number of smaller globes in a more or less fluid condition.

As soon as the different chemical substances came into contact with each other, chemical action of course began, and the heat was raised very considerably; but as the pressure and heat were very different in different parts—of the earth, for instance,—it is possible to understand how the variety of minerals could arise which compose the crust of the earth, and of which it must necessarily be composed in order that organized beings may be able to live upon it. During these great preparations, by which the earth was acquiring its adaption to the future life upon it, the chemical forces were in a great measure converted into heat, a part of which is preserved in the earth so as to keep the temperature of the surface very nearly constant.

At the same time that the material substances were losing their violent characters, the surface of the earth was passing over from the fluid into the solid state to give a field for a vigorous vegetation, which directly appeared as soon as the ground was so far prepared that the plants could meet with the forces pro-



per to the ideal of each individual plant. But, remembering the fact that no force is lost, and that no force can grow up out of nothing, it is clear that in the plants a new arena was opened for the action of the several forces, which from this time were able to take on the forms that give origin to each individual plant.

After the vegetation had prepared the earth's surface sufficiently for animal life to subsist, we find that animal beings living on the plants or at the expense of the energy stored in them were created; and as this is a fact, I think there can be no doubt that the powers of nature henceforth were able, not only to sustain vegetable life and to take on the forms that suit the ideal of each individual plant, but also, through vegetable life, to sustain animal life, and to give to each individual animal its peculiarity of form, activity, strength, and energy; for it must be allowed that neither plants nor animals get their powers out of nothing. By-and-by new animals, living on the former kinds, were created; and after all we find human beings at last created by God. But as it must be granted that every plant, every animal, and of course everything that God has created, is created as a necessary part of a consistent whole, it must be granted likewise that the existence of human life was not possible until the earth was sufficiently prepared and cultivated by the vegetable and animal life which should sustain the life of man; that the intellectual powers of man are but new forms of the powers of nature; that thought itself is intellectual work, which demands its nourishment equally with any other work; and that we cannot be wrong if we consider intellectual life as that form of the natural forces in which they come to an understanding of themselves, their existence and their life—come to an understanding of that spiritual, immaterial, and intellectual power of God which guides nature in its progress towards its final state as intellectual life.

Following the powers of nature in their unfolding, from their original state as closely and intimately connected with matter, till they appear independently of matter, as free, intellectual power, it is interesting to remark not only that, by the direct action of the electrical, chemical, mechanical, and magnetic forces, the quantity of *vis viva* is constantly increasing at the expense of the quantity of energy intimately connected with matter, but also that the same law holds true whenever we arrange it so that the forces must work in the contrary way—for instance, in raising a mass to a certain height, in separating the chemical elements which compose a mineral, &c.—as it is always necessary to employ more power to do the work than we get stored up by the action, and as the surplus of power employed is always liberated in the form of *vis viva*. Consequently the direct

action of the forces of nature is to liberate themselves from intimate connexion with matter ; and whenever we make them act in opposition to this law, we see that we only prepare for a state that will afterwards be destroyed : we can hinder the straight and direct action of the forces to some extent, but we cannot stop it. And as this is a general law of nature for the action of the electrical and chemical, the magnetic and mechanical forces, there is certainly no reason to believe that it should not be a general law for all the forces of nature.

If it be now called to mind that intellectual life commenced with the creation of human beings ;

That human beings did not arise, and therefore must be supposed to be unable to have lived on the earth before it was sufficiently cultivated by numerous animals ;

That a great number of these could not live until numerous other animals, living solely on vegetables, had prepared the earth for them ;

That these animals could not subsist until plants had previously cultivated the ground ;

That many of the plants certainly could not live before other plants had prepared the ground for them ;

That no plants at all would have been able to live had not the ground been sufficiently prepared and cultivated by the action of the chemical forces ; and

That this action could not possibly have taken place had not the chemical forces been called into existence by the energy generated by the original power of gravity ;—if, I say, we call this to mind, I think it cannot fail to strike us, not only that it was necessary for the ground to be thus prepared and cultivated before human beings could live on the earth, but also that it was in like manner necessary that the forces themselves should be prepared and cultivated before they could take on the form of intellectual life.

But if so, I think it must be satisfactory to us to see that, according to the principle of the perpetuity of energy, we arrive at the conclusion that the intelligent life of man must be an intelligent life for ever !

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GENTLEMEN,—After asking for a place in your Journal of Science for this extract or outline of my different memoirs on the forces, I beg leave to trouble you with the question whether, after what I have stated in this extract, you would like to have for your Journal a copy of my first memoir translated into English, as such would enable your readers to judge more fully of my position relatively to the new principle at the time (1843) when I wrote my “Theses concerning Force,” and as it would also

enable them to compare it with the position of M. J. R. Mayer of Heilbronn, in the year before (1842), when he wrote his "Remarks on the Forces of Inanimate Nature." I need scarcely say that such a comparison would be of great interest to me, as I think it would convince your readers of the fact that M. Mayer wrote his "Remarks" in 1842 before he was able to support them by a single experiment, or by anything like a proof of their exactness; whilst I thought it to be my duty, before I wrote, to prove that my suppositions concerning the forces were confirmed by nature itself as a law of nature.

I am, Gentlemen,

Your most respectful and obedient Servant,

A. COLDING,

*Engineer in Chief to the City of Copenhagen.*

## XII. Notices respecting New Books.

*Practical and Spherical Astronomy, for the use chiefly of Students in the Universities. By the Rev. ROBERT MAIN, M.A., F.R.S.* 1863, pp. 392. Cambridge: Deighton, Bell and Co. London: Bell and Daldy.

THIS book must not be confounded with the author's translation of part of M. Brünnow's 'Spherical Astronomy.' It is, in fact, an independent work, and complete according to the author's view of what should be comprised in such a treatise. It contains, in the first place, a full account of the methods employed in modern observatories of making and reducing observations, with particular reference to the methods used at Greenwich (pp. 41, 54, 58, 85, &c.). This is comprised in the first seven chapters. The remainder of the work is of a more miscellaneous character. It is devoted to a discussion of the questions that concern the planets and their satellites, the determination of geographical latitude and longitude, and the prediction of eclipses and occultations. These parts, into which the work might be divided, are of about equal length, the former being somewhat the longer.

It must be understood that the book is intended to give an account of Principles and Methods as distinguished from Results. Accordingly wherever a phenomenon is described it is with the utmost brevity, and such questions as the physical constitution of the moon and planets are noticed only to be dismissed. Yet when results are stated, a great deal of information is conveyed in few words,—*e. g.*, the account of the recent discussion of the sun's horizontal parallax (p. 196); that of the discussion of the acceleration of the moon's mean motion (p. 279); that of Jupiter's satellites (p. 283), &c. In accordance with the same view, the descriptions of the instruments are very brief, while the mathematical questions arising out of their errors are given at full length. There can be no question that Mr. Main is quite right in not describing the instruments with extreme minuteness; still we are compelled to think it a fault that not so much



as an outline figure of a transit-circle or equatorial is contained in the book. The descriptions, it is true, though brief, notice all the essential points, and are perfectly clear if the reader has seen the instruments; but as many persons are interested in the principles of astronomy who have not access to an observatory, we think figures should have been inserted. The best figures and the fullest descriptions are only makeshifts; but though we cannot do as much as might be wished, that is no reason for not doing as much as we can.

The treatise is said to be one of *practical* astronomy. It must be understood that the practice contemplated is that of a fixed observatory. In other words, nautical and geodetical astronomy are not included, though they might with great propriety form part of a treatise of practical astronomy. We are inclined to think that Mr. Main should have observed his own limitation with either more or less strictness. We have failed to find any account, however brief, of the zenith sector, and suppose it was omitted as not being an instrument used in a fixed observatory; yet it is mentioned in connexion with two totally distinct subjects (pp. 160, 166). And, further, in the chapter on the determination of geographical latitude no mention is made of the method depending on the use of that instrument. On the other hand, Mr. Main has noticed the principal problems of sextant astronomy in one part or other of his book, and has investigated the appropriate trigonometrical formulæ. We think it is very much to be regretted that, having given so much of this latter subject, he has not given more. The step which separates the general formula from the numerical result in any particular case is one which the average student would find a difficulty in taking. This is a fact which the student is often unwilling to believe until he is induced to make the attempt, and it is for many reasons desirable that he should make the attempt—an opinion in which we believe Mr. Main shares (pp. 103 (note) and 117). It must be remembered that a student who learns astronomy without access to instruments (and this is the commonest case) is in danger of regarding his text-book as a mere collection of geometrical problems, and, in consequence, of resting content with a vague notion of his subject, such as a student of chemistry would form of his subject if he never made an experiment. We believe that there are few things more calculated to remedy this inconvenience than the actual working out of a few numerical cases of determinations of latitude and local time, starting from the data which would be actually supplied by the sextant and chronometer. All that is wanting for this purpose in Mr. Main's book could be supplied by two or three pages of text, and as many of unworked examples.

Mr. Main has very wisely omitted many problems or propositions which have a sort of traditional right to a place in works of this kind, and has but rarely given an unpractical solution of a problem merely on account of its neatness. We are inclined to think that a few more omissions might be made with advantage, and one or two of his solutions suppressed; instances occur in the chapters on Time and Refraction.



We have thus noticed with some degree of particularity what we are compelled to think weak points in Mr. Main's book, we may therefore with the more freedom express our sense of its many and great merits. The mathematical part is executed with great elegance, and with sufficient, though not too great fulness. This, together with the clearness and brevity of statement exhibited in nearly every page, renders it an admirable text-book, while Mr. Main's eminence as a practical astronomer is sufficient guarantee that the book does really "embody the practice and the theories of the present time." We sincerely congratulate the Students in the Universities that they have here (what they have so long wanted) a thoroughly good text-book on this important and difficult subject.

*The First Principles of Natural Philosophy.* By WILLIAM THYNNE LYNN, B.A., &c. Foolscep 8vo. pp. 100. London: Van Voorst, 1863.

There can be little doubt that the chief value of the study of Physical Science, considered as an agent of general mental culture, is that it affords in a special degree the means of educating the judgment, or the faculty of drawing correct general conclusions from the particular facts of experience. It possesses this value, however, only in so far as it is caused to be an exercise in the experimental method of inquiry, by which alone physical truths can be discovered. In order that physical science may produce its full educational effect, the methods of studying it must be made to reproduce, as nearly as possible, the essential steps of the process of scientific investigation. The physical facts should first be placed clearly before the student; he should then be made to see their mutual connexion and dependence, and whatever regularities they present; and thence he should be led up to the general law by which they are governed. Afterwards should follow the converse process of developing the consequences of the law thus arrived at, and applying the general expression to particular cases. To make the study of Natural Philosophy consist of this latter part only, is to deprive it of all that gives it any distinctive value as a study, and to reduce it to a set of mathematical exercises, the solution of which, however improving, can never impart any other discipline to the mind than that which might be obtained from the solution of any other set of problems involving the same mathematical operations.

Nevertheless, as Natural Philosophy is, and doubtless will continue to be, thus taught, as *merely* a branch of Applied Mathematics, teachers and students must be supplied with books wherein it is so treated. Of books of this class, the one before us is a very favourable specimen. The author has confined himself to the most fundamental points—the first principles—of the subjects treated by him, and these he has succeeded in expounding, for the most part very clearly, within very narrow limits. The work is divided into five sections, respectively devoted to Statics, Dynamics, Hydrostatics and Hydrodynamics, Pneumatics, and Optics, and it is shown how the most important phenomena included under each of

these heads may be deduced from the general laws by which they are governed. The demonstrations are not always complete, but they are short and clear so far as they go. The law of Falling Bodies, as its importance deserves, is treated at greater length than most other subjects, and, we think, very successfully.

In view of a future edition, it is perhaps worth while to call the author's attention to one or two passages where his meaning seems to admit of being made still clearer. For instance, in *Pneumatics*, § 1 (p. 62), we read, "But whereas, in liquids, the pressure exerted is entirely due either to their own weight or to the application of some external pressure, transmitted in this way, the pressure of an elastic fluid depends chiefly upon its elastic force." We would ask—if its own weight and external pressure were removed from an elastic fluid, what would become of its *elastic force*? In the following passage the meaning is plain enough from the context, but the construction seems to imply that the *law of gravitation* is a *force*:—"Before we conclude we will say a few words concerning the law of gravitation. Hitherto we have treated it as if it were a force of always the same amount, &c."

But, notwithstanding such very slight blemishes as these, the author has well executed the task he proposed to himself, and has produced a book which cannot fail to be very acceptable to all who wish to acquire the knowledge he undertakes to convey.

*Complete Solutions of every class of Examples in Algebra, forming a complete Course on the subject, and calculated to facilitate and extend the Study of Mathematics as a logical Course.* By J. WHARTON, B.A., M.C.P., Pp. 430.

We learn from the preface that Mr. Wharton had nearly finished this work at the time of his death, and consequently we do not know whether the title-page is due to the author or editor; but whoever is responsible for it would have done well to have omitted the last clause. Its meaning, though not very obvious, we suppose to be that particular care has been paid to the systematic development of the subject so far as treated in the book: but this is by no means the case; the arrangement of the work is much the same as that generally followed—not bad, nor yet specially good.

The book itself consists of about fifteen hundred examples in Elementary Algebra, together with their Solutions in full. Considerable labour must have been bestowed in collecting or making the examples, and undoubtedly a learner who could solve them all would be well grounded in the Elements of Algebra up to, but not including, the Binomial Theorem. That each example should be followed by its Solution in full seems to us to neutralize completely the value of the collection. The use of an example consists solely in its being done by the learner, not in its being done for him. Some examples, it is true, are so difficult that few learners could solve them without assistance; but in this case a hint, not a solution in full, is the thing wanted. For this reason we will not say more, except to express our regret that the patient and accurate labour expended on the book should have been spent to no useful purpose.





Table of Corrections.

Fect.	<i>c</i>	Diff. for 100 feet.	<i>h''</i> + <i>H''</i> -	Diff. for 100 feet.	<i>L</i> <i>l</i> +	<i>L</i> <i>l</i> -	<i>l</i>
1000	- 0.3	0.06	0.05	0.01	0°	90°	2.65
2000	+ 0.3	0.20	0.20	0.02	5	85	2.61
3000	2.3	0.41	0.43	0.03	10	80	2.49
4000	6.4	0.72	0.77	0.04	15	75	2.29
5000	13.6	1.08	1.20	0.05	20	70	2.03
6000	24.4	1.54	1.72	0.06	21	69	1.97
7000	39.8	2.07	2.35	0.07	22	68	1.91
8000	60.5	2.68	3.06	0.08	23	67	1.84
9000	87.3	3.35	3.88	0.09	24	66	1.77
10000	120.8	4.16	4.79	0.10	25	65	1.70
11000	162.4	5.04	5.79	0.11	26	64	1.63
12000	212.8	6.01	6.89	0.12	27	63	1.56
13000	272.9	7.07	8.09	0.13	28	62	1.48
14000	343.6	8.27	9.38	0.14	29	61	1.40
15000	426.3	9.55	10.77	0.15	30	60	1.33
16000	521.8	10.98	12.26	0.16	31	59	1.24
17000	631.6	12.40	13.84	0.17	32	58	1.16
18000	755.6	14.35	15.51	0.18	33	57	1.08
19000	899.1	16.05	17.28	0.19	34	56	0.99
20000	1059.6	18.03	19.15	0.20	35	55	0.91
21000	1239.9	20.23	21.11	0.21	36	54	0.82
22000	1442.2	22.56	23.17	0.22	37	53	0.73
23000	1667.8	25.14	25.33	0.23	38	52	0.64
24000	1919.2	27.96	27.58	0.24	39	51	0.55
25000	2198.8	31.00	29.92	0.24	40	50	0.46
26000	2508.8	34.65	32.36	0.25	41	49	0.37
27000	2852.3	37.96	34.90	0.26	42	48	0.27
28000	3231.9	41.99	37.53	0.27	43	47	0.18
29000	3651.8	46.36	40.26	0.28	44	46	0.09
30000	4115.4		43.09		45	45	0.00

Ex. 1. Height of Mont Blanc above Geneva from the observations of MM. Bravais and Martins, August 29th, 1844.

$$\begin{array}{rclcl}
 A' & 19.3 & B & 729.65 \text{ mm.} & M' & 18.6 & H & 1335.33 \\
 a' & - 7.6 & b & 424.05 & m' & - 4.2 & L & 46 \\
 & 500.0 & B+b & 1153.70 & & 22.8 & l & 0.09 \\
 & 511.7 & B-b & 305.60 & & \times 4.23 & & \times 14\frac{1}{2} \\
 & & & & p & 96.4 & q & 1.3
 \end{array}$$

$$\begin{array}{rcl}
 305.6 \times 52400 \div 1153.7 & 14118.1 \times 511.7 \div 500 \\
 = 13880.0 & = 14448.5 \\
 \begin{array}{l} 272.9 \text{ } c \text{ for } 13000 \\ 61.6 \text{ diff. for } 880 \\ \hline 103.6 \text{ } -p \end{array} & \begin{array}{l} 10.8 \text{ } h'' \text{ for } 15000 \\ 1.2 \text{ diff. for } 800 \\ \hline 1.9 \text{ } -H'' \text{ for } 1500, H \\ \hline 18.7 \text{ } -q \end{array} \\
 14118.1 & &
 \end{array}$$

$$h - H = 14459.0 \text{ difference of level.}$$

Result by Laplace's formula 14459.4.

*Ex. 2.* Rush's balloon ascent, September 10th, 1838 (see Meteorological Papers by Admiral FitzRoy, No. 9, p. 19).

A	60	B'	30.496 in.	H	0
a	5	b'	10.830	L	52
	836	B'+b'	41.326	l	0.64
	901	B'-b'	19.666		$\times 27$
				q	17.3
$19.666 \times 52400 \div 41.326$			$27116 \times 901 \div 900$		
$=24935.8$			$=27146.1$		
2198.8 c for 25000			34.9 h'' for 27000		
181.4 diff. for -65			0.3 diff. for 100		
$27116.0$			$182.7 - q$		
			$h-H=27164.0$		

Laplace's formula gives the same result.

As the British highlands do not exceed 5000 feet in altitude, and lie near the parallel of  $56^\circ$  north latitude, the corrections will nearly destroy each other. The following simple rule will therefore suffice for calculating all British heights:—

“Multiply the difference of the barometers by 524, and divide the product by the sum of the barometers, retaining three decimal places. Multiply this quotient by the sum of the temperatures of the air increased by 836, and divide the product by 9, keeping one decimal place. For aneroid and corrected mercurial barometers, the quotient is the height in English feet. For uncorrected barometers, subtract  $2\frac{1}{2}$  times the difference of the temperatures of the mercury.”

*Ex. 3.* Height of Ben Lomond (see Col. Sir H. James's Instructions for taking Meteorological Observations, App.).

A	59.0	B	29.890 in.	M	60.8
a	47.8	b	26.656	m	49.3
	836.0	B+b	56.546	M-m	11.5
	942.8	B-b	3.234		$\times 2\frac{1}{2}$
					28.7

$$3.234 \times 524 \div 56.546 \times 942.8 \div 9 - 28.7 = 3110.5 = h - H.$$

The height by Laplace's formula is 3110.8, by levelling 3115.8. The accuracy of the present formula is only intended to be tested by Laplace's, and it will be wrong to at least the same extent.

Very good results may also be obtained by neglecting  $H''$ , which is always very small, and transposing the terms  $h''$  and  $-2.35(M-m)$ ; thus

$$h-H = \left(52400 \frac{B-b}{B+a} + c + h''\right) \cdot \frac{836+A+a}{900} + .001 \cdot (h-H) l - 2\frac{1}{2}(M-m),$$

where  $2\frac{1}{2}$  is written for 2.35 to compensate for omitting to multiply the latter by  $(836+A+a) \div 900$ . This approximate form gives rise to the following practical rule for determining heights under 10,000

feet, embodying so much of the Table of corrections as is necessary for that purpose.

“Multiply the difference of the barometers by 52400, and divide by the sum of the barometers. If the number of clear thousands in the quotient be

1,	2,	3,	4,	5,	6,	7,	8,	9,	10,
add 0,	0·5,	2·7,	7·2,	14·8,	26·1,	42·2,	63·6,	91·2,	125·6
and	0·2,	0·5,	0·8,	1·1,	1·6,	2·1,	2·9,	3·1	

for every additional hundred. Then multiply the result by the sum of the temperatures of the air increased by 836, and divide the product by 900. To this quotient

add for lat. . . .	0,	10,	20,	30,	32,	34,	36,	38,	40,	42,	44,
subtract for lat. .	90,	80,	70,	60,	58,	56,	54,	52,	50,	48,	46
the numbers. . .	2·6,	2·5,	2·0,	1·3,	1·2,	1·0,	0·8,	0·6,	0·5,	0·3,	0·1

for every clear thousand it contains. For aneroid and corrected mercurial barometers this result is the height in English feet. For uncorrected mercurial barometers, subtract  $2\frac{1}{2}$  times the difference of the temperatures of the mercury.

“The barometers may be expressed in any units. If the temperatures are expressed in

degrees Centigrade, use.....	500,	500,	$4\frac{1}{2}$ ,
degrees Reaumur, use .....	400,	400,	$5\frac{1}{2}$ ,
in place of.....	836,	900,	$2\frac{1}{2}$ ,

which are only suited for degrees Fahrenheit. The rule and the other numbers remain unaltered, and the result is in English feet.”

*Ex.* 4. Height of Guanaxuato in Mexico.

A 77·5	B 30·046	M 77·5	L 21
a 70·3	b 23·660	m 70·3	l 2·0
836·0	B + b 53·706	M - m 7·2	× 6·8
983·8	B - b 6·386	2½	q 13·6
		p 18·0	

$$6\cdot386 \times 52400 \div 53\cdot706 = 6230\cdot7$$

$$26\cdot1 \text{ for } 6000$$

$$3\cdot7 \text{ diff. for } 230$$

$$6260\cdot5$$

$$6260\cdot5 \times 983\cdot8 \div 900 = 6843\cdot4$$

$$13\cdot6 \quad q$$

$$182\cdot0 - p$$

$$h - H = 6839\cdot0$$

Result by Laplace's formula 6838·2.

These results are obtained by transforming Laplace's formula as follows. The original expression in the *Méc. Cél.* vol. iv. p. 293, reduced to English measures and the present notation, is



$$h-H=60158.71 \cdot (1+0.002845 \cos 2L) \cdot \frac{836+A+a}{900} \times \left[ \left( 1 + \frac{h-H}{R+H} \right) (\log B' - \log b') + \frac{h-H}{R+H} \cdot 0.868589 \right] \quad \dots (b)$$

which Delcros has transformed (in the 'Annuaire Météorologique de la France' for 1849) to the equivalent of

$$h-H=60158.71 \times [\log B - \log b - 0.0000389278 \cdot (M-m)] \times \frac{836+A+a}{900} \times G \times \left[ 1 + \frac{h-H+52251}{R} + \frac{H}{\frac{1}{2}R} \right] \quad \dots (c)$$

The last factor may be split into the two

$$\left( 1 + \frac{52251}{R} \right) \cdot \left( 1 + \frac{h+H}{R} \right)$$

without sensible error. Then, since

$$60158.71 \times \left( 1 + \frac{52251}{R} \right) = 60309.19$$

and  $60309.19 \times 0.0000389278 = 2.34770$ ,

if we put  $h-H$  for the product of the three first factors on the right-hand side in (c), we find

$$h-H = [60309.19 \cdot (\log B - \log b) - 2.34770 \cdot (M-m)] \cdot \frac{836+A+a}{900} + \frac{h-H}{1000} \times 2.6257 \cos 2L + \frac{h^2-H^2}{R} \quad \dots (d)$$

Putting 2.35 for 2.34770, and  $l, h'', H''$  for their values, this form (d) will be identical with (a), provided that

$$60309.19 \cdot (\log B - \log b) = 52400 \cdot \frac{B-b}{B+b} + c \quad \dots (e)$$

Now putting  $B-b=yB$ , we have

$$\frac{B-b}{B+b} = \frac{y}{2-y} = \frac{1}{2} \cdot \left( y + \frac{1}{2}y^2 + \frac{1}{4}y^3 + \frac{1}{8}y^4 + \dots \right) = \frac{1}{2}z,$$

$$\log B - \log b = \log \frac{1}{1-y} = \mu \cdot \left( y + \frac{1}{2}y^2 + \frac{1}{3}y^3 + \frac{1}{4}y^4 + \dots \right) = \mu(z+d),$$

where  $\mu$  is the modulus of the tabular logarithms, and

$$d = \frac{1}{12}y^3 + \frac{1}{8}y^4 \dots,$$

always a convergent series as  $y$  is always a proper fraction, and small when  $y$  is small, as it is for moderate heights.

Hence

$$\begin{aligned} 60309.19 \cdot (\log B - \log b) &= 60309.19 \times 2\mu \frac{B-b}{B+b} + 60309.19 \cdot \mu d \\ &= 52384 \frac{B-b}{B+b} + c'. \end{aligned}$$

The constant 52384 has been changed to 52400 to facilitate calculation and to divide the correction for the first two thousand feet, and  $c'$  has consequently been altered to  $c$ , the tabular values of which were calculated as follows.

Put  $x = 52400 \frac{B-b'}{B+b'} = 52400 \cdot \frac{y}{2-y},$

whence

$$y = \frac{2x}{52400 + x} \cdot \cdot \cdot \cdot \cdot \cdot \cdot (f)$$

Then (e) becomes

$$x + c = 60309 \cdot 19 \cdot (\log B - \log b) = -60309 \cdot 19 \log (1 - y) \quad (g)$$

Make  $x$  successively = 1000, 2000, &c. up to 30,000, and find the corresponding values of  $y$  from (f) and  $c$  from (g).

As the differences in the values of  $c$  are not uniform, slight errors may arise from neglecting second differences in interpolation, but they can scarcely ever affect the result by a single unit, and may therefore be safely disregarded. Laplace's formula itself cannot be depended on within much larger limits.

The Table of corrections and transformation of Laplace's formula here given allow of the following simplification in the logarithmic calculation of  $h-H$ .

$$\begin{aligned} \text{Let } \log n &= \log [\log B - \log b - 0.00004 \cdot (M - m)] \\ &\quad + 1.8261420 + \log (836 + A + a) \\ &= \log [\log B - \log b - 0.00007 \cdot (M' - m')] \\ &\quad + 2.0814145 + \log (500 + A' + a') \\ &= \log [\log B - \log b - 0.00009 \cdot (M'' - m'')] \\ &\quad + 2.1783245 + \log (400 + A'' + a''), \end{aligned}$$

then  $h - H = n + \cdot 001 . nl + h'' - H''$ ,

where  $1.8261420 + \log 900 = 2.0814145 + \log 500$   
 $= 2.1783245 + \log 400 = 4.7803845 = \log 60309.19.$

This form requires less previous preparation, avoids the logarithms of numbers near to unity as  $\left(1 + \frac{A+a-64}{900}\right)$ , and allows of the use of foreign data to obtain the result in English feet, so that it only becomes necessary to reduce the height of the lower station to English measures.

GEOLOGICAL SOCIETY.

[Continued from vol. xxvi, p. 553.]

December 2, 1863.—Professor A. C. Ramsay, President,  
in the Chair.

The following communications were read:—

1. "On the Correlation of the Oligocene Deposits of Belgium, Northern Germany, and the South of England." By Herr Adolf von Koenen. Communicated by F. E. Edwards, Esq., F.G.S.

Railway-cuttings in the New Forest (Brockenhurst, &c.) have

recently exposed certain marine beds overlying the Lower Headon (freshwater) series, and containing fossils hitherto unknown in England, but which, as Herr von Koenen showed, constitute the marine equivalent of the Middle Headon strata.

The author gave an exposition of the current opinions upon the correlation of those English and Foreign "Upper Eocene" or "Lower Miocene" strata, to which Professor Beyrich has given the name "Oligocene," and briefly sketched their distribution and limits upon the continent. He then gave a list of fifty-nine New-Forest (Middle Headon) fossils, which he had determined, and stated that, of this number, forty-six occur in the Lower Oligocene of Germany, and twenty-three are characteristic of that formation; twenty-one of these species occur in the Barton Clay, four in the Middle Oligocene, and eight are peculiar to the Brockenhurst beds. He therefore concluded that the Headon and Brockenhurst strata are on the same horizon as the Lower Oligocene; and he confirmed the opinion of previous observers—that the Hempstead beds are the equivalent of the "Grès de Fontainebleau" and of the Middle Oligocene of Germany.

2. "On the Liassic Strata of the Neighbourhood of Belfast." By Ralph Tate, Esq., F.G.S.

In the neighbourhood of Belfast the following members of the Lias formation were stated to occur, namely:—The Zone of *Ammonites Bucklandi*, the White Lias, and the Zone of *Avicula contorta*.

The characters of these subdivisions in the district under consideration were described in detail by Mr. Tate, who gave sections of the beds exposed in Colin Glen and at Cave Hill, at which localities the three zones are seen; he also gave sections of the *Avicula contorta* beds as exposed at Woodbarn and at Whitehead, and lists of the fossils found in the strata of each subdivision at the localities mentioned, noticing that, in the zone of *Ammonites Bucklandi*, that Ammonite is replaced by *A. intermedius*, the other fossils being of the same species as occur in that zone in England; and he concluded with some general remarks on the distribution of the members of the Lias in the North of Ireland.

3. "Notes on the Devonian Rocks of the Bosphorus." By W. R. Swan, Esq. In a letter to Sir R. I. Murchison, K.C.B., F.R.S., F.G.S., &c.

The lithological and stratigraphical characters of the rocks of the Bosphorus having been noticed, the author gave a general description of the fossils occurring in them,—namely, *Spirifer* (broad-winged and small species), *Orthis*, and other Brachiopods; *Homalonotus* and other Trilobites; together with Corals of the genus *Favosites* associated with the well-known *Pleurodictyum problematicum*. Graptolites were stated to be entirely wanting, and Cephalopods to be very rare. Mr. Swan therefore inferred that these strata were of the age of the Lower Devonian rocks of the Rhine.

XIV. *Intelligence and Miscellaneous Articles.*

REMARKS ON THE THEORY OF GASES. BY J. STEFAN.

ACCORDING to the new theory of gases a local excess of temperature in a gas ought to disappear from its place almost instantaneously; and on many sides this has been urged as an objection to the theory. The objection was based on the consideration of the simple case of the propagation of *vis viva* in a series of equally large elastic spheres. Each sphere on a central impact with the next one interchanges velocity with it, and their excess of *vis viva* propagates itself through the entire series of spheres with their velocity. The new theory gives very large values for the velocities with which the gas molecules progressively move, by which the above objection seemed justified.

Clausius has answered this in his theory of conduction in gases. He rejects the above simple case of the propagation of *vis viva* as totally useless, even approximately, for the purpose of leading to a conclusion on the point in question. On the contrary, Clausius lays considerable weight, and justly, on the consideration of the irregular motions of the molecules. His calculation based upon this gives a very small value for the conducting power of gases. This result he adduces in refutation of the above objection. According to his view it appears as if this result were dependent on the consideration of irregular motions. But even under the supposition of a regular arrangement of the molecules, a very small value is obtained for the conducting power, as can readily be shown without tedious calculation.

If we imagine two superincumbent layers of gas with a common base 1, and each of the height  $h$ . Let the temperature of the higher layer be  $h$  degrees higher than the lower one. If  $c$  is the specific heat of the gas for a constant volume,  $s$  its specific gravity, the upper layer has an excess of temperature whose magnitude is  $cs h^2$ . If we divide the molecules of a layer into two parts, into those with a predominating upward motion, and those with a predominating downward one, then the latter part of the molecules in the upper layer will transfer half their excess of temperature to the downward moving molecules of the lower layer in the time in which a molecule traverses a layer. The thickness of the layer is then equal to the mean of the path of a molecule from one impact to the other, to be taken in the direction normal to the planes of the layers. If  $\tau$  is the time which the molecule requires to traverse the layer,  $\frac{cs h^2}{2\tau}$  is the quantity of heat given by the upper to the lower layer, reduced to the unit of time. Hence this magnitude must be considered as an approximate expression for the conductivity of a gas. Clausius finds for it  $\frac{Scse^2}{12\tau}$ , in which  $e$  is the mean path of a molecule from one impact to the next, and  $\tau$  the time taken. It is already evident that, on the supposition of a given arrangement of the gas molecules, values will always be obtained for the conductivity of gases which will be of the same order as those found by Clausius. By proving



the smallness of the conducting-power of gases, the above objection is not refuted. For the case in which the consideration of the regular arrangement of the molecules is taken as basis, it is plain that the excess of temperature must travel from one layer to another with a velocity which cannot be different from that of sound. And the same follows also from the investigation of the irregular motions.

But even if heat is propagated in gases with the same velocity as sound, the above objection is still not justified. It will only be so when it necessarily follows from the new theory of gases, that an excess of temperature in a given place of the gas is not only very rapidly, but also *completely* and *without loss* imparted to the neighbourhood. On the other hand, the objection is refuted if it can be shown that the influence of an increase of temperature extends indeed in a short time to great distances, but that the force of this influence is continually weaker with an increase of distance, and for a short time can be infinitely small, even for small distances.

Theory leads to this result when it is remembered that the motions of gas molecules take place in all possible directions, and, *what is essential, that this irregularity has its reason in the oblique impacts on each other of the molecules.* For on oblique impact one molecule does not interchange its velocity with another; the entire excess of one molecule is not transferred to another, but in the mean only one-half, assuming that the directions of the motions of the molecules towards the line which joins their centres at the moment of impact have all possible inclinations. In this consists the special peculiarity of the conduction of heat, the tendency towards an equalization of temperature. Hence of the downward moving molecules of the upper layer, the entire excess of temperature in them will not be transferred to the lower one, but only one-half; that is  $\frac{1}{2}$  of the entire excess of the layer: from this to the next there will only be  $\frac{1}{4}$  of the original excess of the first layer, and so on. If even the excess of temperature after  $\frac{1}{2}$  of a second prevails to the distance of a metre, this influence must be infinitely small, if we consider how great is the number of layers into which, according to Maxwell's calculation of the mean path of a molecule, a gas layer a metre in thickness must be divided. Hence a considerable time must elapse before the *vis viva* which has traversed a metre amounts to so much as to be perceptible.

The following conclusion arises from what has been said. The velocity of the propagation of sound and of heat, that is to say conduction, are equal. The propagation of sound is distinguished from that of heat by the fact that in the first case the difference of *vis viva* produced anywhere by contraction or expansion is imparted without loss to the adjacent mass, *while this is not the case in the propagation of heat, with the excess of vis viva produced by increase of temperature, inasmuch as only one-half of this is imparted to the neighbourhood.*

The assertion that heat has as great a velocity of propagation as sound, appears contradictory to experiment. This is not the case. Let us consider, for example, the following simple case of propagation.

One end of a bar, the entire length of which is originally at  $0^{\circ}$ , is suddenly heated to  $100^{\circ}$  and kept at this temperature. If a place is observed which is 1 metre distant from the end, it will after an hour assume a temperature of  $50^{\circ}$ . But the same place had previously the temperature of  $40^{\circ}$ , before that the temperature  $30^{\circ}$ , still earlier the temperature  $20^{\circ}$ , and so on. Reasoning further from these data, we get the result, that an infinitely small increase of temperature at this place must have occurred after an infinitely short time—that thus the velocity of the propagation of heat is very large: it is even found to be infinitely large if the formula given for this case in Fourier's theory of conduction is used. Hitherto neither experiment nor theory have given a useful datum to the question of the velocity of the propagation of heat.

The difference between the propagation of sound and of heat is readily understood from the new theory of gases. A condensation is suddenly produced in a gas layer. To exclude a simultaneous increase of the velocity of the molecules, we must suppose it to be produced by the fact that in this layer several molecules are placed which possess velocities equal to those present. Obviously more molecules emerge from this layer into the adjacent ones than the reverse; the condensation passes into these, from these to the next, and so on, without being enfeebled during the propagation, while from the equal velocities of the molecules, the oblique impacts are without that influence on the distribution of *vis viva* which they have in the propagation of an increase of temperature.—*Berichte der Wiener Akademie*, vol. xlvii.

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MR. W. THOMSON'S METHOD FOR MEASURING ELECTRICAL CONDUCTIVITY—APPLICATION TO FUSED METALS. BY M. DE LA RIVE.

To Mr. W. Thomson is due a new method of measuring electrical conductivity. By using an arrangement of conductors only one of which is the seat of an electromotive force, the intensity of the current in a galvanometer wire may be made to depend on the ratio of two electrical resistances which are compared. In this arrangement, the two ends of the galvanometer wire terminate in two points taken on two conductors of the system, and these points are determined so as to divide in the same ratio the total resistances of these two conductors.

Now it is seen that if this same ratio exists between two other resistances, which are, on the one hand, the unknown resistance, and on the other that which is taken as unity, the intensity of the current in the galvanometer is zero. The method consists then in varying the known resistance until the current is annulled.

The principle is the same as in Wheatstone's method, which may in fact be considered as resulting from a particular case of the more general method devised by Mr. Thomson. From the complication of this method, that of Wheatstone will usually be preferred; but the latter becomes inadequate when the resistances to be measured are small as compared with the accessory resistances, while in Thom-

son's method the absolute value of the unknown does not affect the exactitude of the determination. This difference is explained by the following remark. In Mr. Wheatstone's method, the equation by which the unknown is determined is  $\frac{x+b}{a}=k$ ; where  $x$  is the unknown

resistance,  $b$  the resistance of the conductors which join  $x$  to the system,  $a$  the standard resistance, and  $k$  the numerical ratio whose value depends on the reading of the rheostat. When  $x$  diminishes in reference to  $b$ , the error relative to  $x$  increases, that of  $k$  remaining constant. In Thomson's method,  $k$  is determined by the

value of  $a$  resulting from observation, and the equation  $\frac{x}{a}=k$ ;

the error relative to  $x$  is constant with that of  $a$ . The resistances arising from imperfect contacts are also, in Wheatstone's method, a source of errors which are more considerable in proportion as the absolute value of  $x$  is small. In Thomson's method, the junctions of the conductors may be arranged so as to throw these two indeterminate resistances, not upon  $x$  and  $a$ , but upon two resistances the division of which on the wire of the galvanometer determines the value of  $k$ . If by  $R$  and  $kR$  we designate the two parts into which each of these resistances is divided, we see that by giving to  $R$  a very considerable value, the resistances of contact do not appreciably

alter the relation  $\frac{kR}{R}$ . Finally, analogous consideration shows that

the use of this method is still more advantageous in the case in which the conductor whose resistance is to be measured is raised to a very high temperature; for since the conductors which join it to the rest of the system necessarily become heated, and in the equation

$\frac{x+b}{a}=k$  of Wheatstone's method the quantity  $b$  is variable,

in Thomson's method, by assigning to  $R$  and  $kR$  a high value, the variation arising from the heating of the extremities is inappreciable.

The conditions which render Thomson's method superior to that of Wheatstone are precisely those met with in the measurement of fused metals. For (1) the unknown resistance is small as compared with the accessory resistances, since on the one hand a column of fused metal can neither be long nor of small diameter, and on the other hand the furnace must be at some distance from the measuring apparatus; (2) there are variable resistances of contact; (3) the conductor is at a high temperature. The experiments which I have made in the chemical laboratory of the Ecole Normale, under the valuable and friendly direction of M. Ste.-Claire Deville, are of a kind to show that the method is suited to researches of this kind.

*Apparatus.*—I had two special instruments made, a *rheostat* and what may be called a compensator. These two apparatus are from the workshop of M. Fromart. The rheostat consists essentially of a metallic wire stretched along a divided rule on which slide two rollers, each with a platinum plate which rests at its edge on the wire. The part of the wire between the two plates forms the



resistance  $a$ , and by means of the divided rule it may be measured almost to the tenth of a millimetre. The compensator serves to determine the value of the ratio  $k$ . This apparatus consists of two identical series of bobbins formed of german silver wire of 0.17 millim. in diameter, and offering great resistance. Each series consists of nine bobbins; the first constitutes the resistance  $R$ ; and by the rest, to a greater or less extent,  $kR$  may be constituted, and any values given to  $k$  from 1 to 50. A Ruhmkorff's galvanometer with a long wire was placed at about 2 metres distance from the rheostat, and was observed by means of a horizontal telescope and a reflecting prism. By means of a key the current could be instantaneously closed. By means of insulated wire, the rheostat, the compensator, and the galvanometer could be connected either with each other, or with a battery consisting of 2 or 3 Bunsen's cells (which was placed under a shed), or, finally, with the resistance to be measured, which was placed in a furnace-room. To hold the melted metals, I used Bayeux porcelain U-tubes from Mr. Gosse's manufactory. These tubes are 25 centimetres in length by 5 millimetres internal diameter; their two parallel legs terminate in cylindrical cups 2 centimetres in diameter and 4 in height. To effect the fusion of the metals and retain them at a constant known temperature, I used vapour baths such as were employed by MM. Ste.-Claire Deville and Troost in their researches on the density of vapours; the tube was suspended in the interior of the vessel in which the distillation took place. Boiling mercury, sulphur, and cadmium were used—that is to say, temperatures of 358, 440 and 860 degrees.

*Conductibility of a melted metal.*—At first the resistance of a tube full of mercury was measured; then, this tube being arranged in the retort and filled with metal, the resistance was measured during the distillation of the substance employed. This resistance remained constant in the vapour of mercury, and also, though to a less extent, in that of cadmium; but the density of sulphur is too small to maintain a constant temperature in a metallic mass, and the results thus obtained leave much uncertainty as far as temperature is concerned. A determination is obtained from the mean of two observations, differing from one another by the relative positions of the two series of bobbins of the compensator.

*Change of resistance in the passage from the solid to the liquid state.*—The metallic column was allowed to cool and a series of observations made noting the corresponding times. Curves were thus obtained whose ordinates were the resistance and whose abscissæ the time, and on which the passage from one state to the other is an almost vertical branch. The results deduced from these curves, taking the two points where the curvature changes to determine the resistances at the point of fusion, may be considered, in consequence of the magnitude of the variation, as being the approximate measure.

The metals are—*tin* (stannic acid reduced by charcoal), *lead* (acetate of lead calcined), *bismuth* (subnitrate of bismuth reduced by charcoal), *cadmium* (distilled cadmium), *zinc* (distilled zinc), and *antimony* (tartarized antimony calcined with nitre). The mercury used had stood for some time under sulphuric acid. The conductivities refer to pure mercury at 21 degrees.



	Temperature.		Conductivity.	
Tin .....	358°	First experiment. . .	1·89	
		Second experiment. .	1·88	
		Mean .....	1·88	
	860°	One experiment ....	1·42	
	Fusing-point.	Liquid state .....	2·0	
		Solid state .....	4·4	
	358°	First experiment. . .	0·715	
		Second experiment . .	0·697	
Mean .....		0·70		
Bismuth ..	860°	One experiment ....	0·596	
		Fusing-point.	Liquid state .....	0·73
	Solid state .....		0·34	
	Zinc .....	440°	One experiment ....	2·58
Fusing-point.			Liquid state.....	2·6
			Solid state .....	5·2
Lead.....		358°	First experiment. . .	0·951
	Second experiment..		0·966	
	Mean .....		0·958	
		860°	One experiment ....	0·771
Fusing-point.			Liquid state.....	1·0
		Solid state .....	1·09	
Cadmium ..		440°	First experiment....	2·79
	Second experiment..		2·46	
	Mean .....		2·62	
	Fusing-point	Liquid state.....	2·8	
Solid state .....		5·0		
Antimony..	860°	First experiment....	0·790	
		Second experiment..	0·776	
		Mean .....	0·783	
	Fusing-point.	Liquid state.....	0·84	
Solid state .....		0·59		

Neither zinc nor cadmium could be fused in the vapour of cadmium, in consequence of oxidation.

*Conclusions.*—It has been confirmed for tin, lead, bismuth, and antimony, that the resistance increases from the fusing-point to the highest limit which could be obtained. The total increase, corresponding to 502°, between 358° and 860°, divided by the resistance at 358°, is 0·32 for tin, 0·24 for lead, and 0·18 for bismuth—values which differ considerably, and are all smaller than that found for mercury, using its known coefficient.

For all these metals there is a sudden change corresponding to the change of condition. For tin, lead, cadmium, and zinc the resistance is almost doubled; for bismuth and antimony the variation is inverse, greater for bismuth than for antimony.—*Comptes Rendus*, Oct. 26, 1863.

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FEBRUARY 1864.

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XV. *Speculative Ideas respecting the Constitution of Matter.*

By T. GRAHAM, F.R.S.\*

IT is conceivable that the various kinds of matter, now recognized as different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies. We know the anxiety with which this point was investigated by Newton, and the care he took to ascertain that every kind of substance, "metals, stones, woods, grain, salts, animal substances, &c.," are similarly accelerated in falling, and are therefore equally heavy.

In the condition of gas, matter is deprived of numerous and varying properties with which it appears invested when in the form of a liquid or solid. The gas exhibits only a few grand and simple features. These again may all be dependent upon atomic and molecular mobility. Let us imagine one kind of substance only to exist, ponderable matter; and further, that matter is divisible into ultimate atoms, uniform in size and weight. We shall have one substance and a common atom. With the atom at rest the uniformity of matter would be perfect. But the atom possesses always more or less motion, due, it must be assumed, to a primordial impulse. This motion gives rise to volume. The more rapid the movement the greater the space occupied by the atom, somewhat as the orbit of a planet widens with the degree of projectile velocity. Matter is thus made to differ only in being lighter or denser matter. The specific

\* From the Proceedings of the Royal Society, 1863, p. 620. Communicated by the Author.

motion of an atom being inalienable, light matter is no longer convertible into heavy matter. In short, matter of different density forms different substances—different inconvertible elements as they have been considered.

What has already been said is not meant to apply to the gaseous volumes which we have occasion to measure and practically deal with, but to a lower order of molecules or atoms. The combining atoms hitherto spoken of are not therefore the molecules the movement of which is sensibly affected by heat with gaseous expansion as the result. The gaseous molecule must itself be viewed as composed of a group or system of the preceding inferior atoms, following as a unit laws similar to those which regulate its constituent atoms. We have indeed carried one step backward, and applied to the lower order of atoms, ideas suggested by the gaseous molecule, as views derived from the solar system are extended to the subordinate system of a planet and its satellites. The advance of science may further require an indefinite repetition of such steps of molecular division. The gaseous molecule is then a reproduction of the inferior atom on a higher scale. The molecule or system is reached which is affected by heat, the diffusive molecule, the movement of which is the subject of observation and measurement. The diffusive molecules are also to be supposed uniform in weight, but to vary in velocity of movement, in correspondence with their constituent atoms. Accordingly the molecular volumes of different elementary substances have the same relation to each other as the subordinate atomic volumes of the same substances.

But further, these more and less mobile or light and heavy forms of matter have a singular relation connected with equality of volume. Equal volumes of two of them can coalesce together, unite their movement, and form a new atomic group, retaining the whole, the half, or some simple proportion of the original movement and consequent volume. This is chemical combination. It is directly an affair of volume, and only indirectly connected with weight. Combining weights are different because the densities, atomic and molecular, are different. The volume of combination is uniform, but the fluids measured vary in density. This fixed combining measure—the *metron* of simple substances—weighs 1 for hydrogen, 16 for oxygen, and so on with the other “elements.”

To the preceding statements respecting atomic and molecular mobility, it remains to be added that the hypothesis admits of another expression. As in the theory of light we have the alternative hypotheses of emission and undulation, so in molecular mobility the motion may be assumed to reside either in separate atoms and molecules, or in a fluid medium caused to undu-

late. A special rate of vibration or pulsation originally imparted to a portion of the fluid medium enlivens that portion of matter with an individual existence, and constitutes it a distinct substance or element.

With respect to the different states of gas, liquid, and solid, it may be observed that there is no real incompatibility with each other in these physical conditions. They are often found together in the same substance. The liquid and the solid conditions supervene upon the gaseous condition rather than supersede it. Gay-Lussac made the remarkable observation that the vapours emitted by ice and water, both at  $0^{\circ}$  C., are of exactly equal tension. The passage from the liquid to the solid state is not made apparent in the volatility of water. The liquid and solid conditions do not appear as the extinction or suppression of the gaseous condition, but something *superadded* to that condition. The three conditions (or constitutions) probably always coexist in every liquid or solid substance, but one predominates over the others. In the general properties of matter we have, indeed, to include still further (1) the remarkable loss of elasticity in vapours under great pressure, which is distinguished by Mr. Faraday as the Cagnard Latour state, after the name of its discoverer, and is now undergoing an investigation by Dr. Andrews, which may be expected to throw much light upon its nature; (2) the colloidal condition or constitution, which intervenes between the liquid and crystalline states, extending into both and affecting probably all kinds of solid and liquid matter in a greater or less degree. The predominance of a certain physical state in a substance appears to be a distinction of a kind with those distinctions recognized in natural history as being produced by unequal development. Liquefaction or solidification, therefore, may not involve the suppression of either the atomic or the molecular movement, but only the restriction of its range. The hypothesis of atomic movement has been elsewhere assumed, irrespective of the gaseous condition, and is applied by Dr. Williamson to the elucidation of a remarkable class of chemical reactions which have their seat in a mixed liquid.

Lastly, molecular or diffusive mobility has an obvious bearing upon the communication of heat to gases by contact with liquid or solid surfaces. The impact of the gaseous molecule upon a surface possessing a different temperature, appears to be the condition for the transference of heat, or the heat movement, from one to the other. The more rapid the molecular movement of the gas, the more frequent the contact, with consequent communication of heat. Hence, probably, the great cooling-power of hydrogen gas as compared with air or oxygen. The gases named have the same specific heat for equal volumes; but



a hot object placed in hydrogen is really *touched* 3·8 times as frequently as it would be if placed in air, and 4 times as frequently as it would be if placed in an atmosphere of oxygen gas. Dalton had already ascribed this peculiarity of hydrogen to the high “mobility” of that gas. The same molecular property of hydrogen recommends the application of that gas in the air-engine, where the object is to alternately heat and cool a confined volume of gas with rapidity.

# XVI. *On the Dynamical Theory of Heat.*

By JOSEPH GILL, *Esq.*

*To John Tyndall, Esq., F.R.S. &c.*

SIR,

THE kind courtesy with which you received my letter “On the Dynamical Theory of Heat,” inserted in the *Philosophical Magazine* for August last, encourages me to hope that you will be so good as to give to the following remarks, in continuation of the subject, an opportunity of meeting the notice of scientific men through the same channel. My only object is to elicit truth. I have neither time nor inclination nor ability for scientific controversy, and I long shrunk back from the idea of making public the results of my researches on a subject to which I have given much attention during a large portion of the term of a medium lifetime; but at length my conviction of the truth of some of the glimpses of light which have been afforded me in these intricate researches forced itself into utterance; and my anxiety to see the difficulties which have baffled me removed by abler hands has caused me to persevere in the effort to induce some one of the eminent men who have distinguished themselves in the development of the new science of thermo-dynamics to reconsider these knotty points, and clear away if possible, once for all, the doubts and obscurity which still cloud some parts of the subject.

Since the date of my last letter I have read your excellent “*Lectures on Heat as a mode of Motion*,” and I feel that the highest praise is due to the ability with which the subject has been divested of technical and mathematical intricacies, and brought within the grasp of mind of every earnest reader of average intellect. By thus exhibiting the results of deep and involved researches in “that simple form in which great truths present themselves to those who thoroughly understand them,” an inestimable boon is conferred on numerous readers possessing a knowledge of physics quite sufficient to handle subjects fairly within the range of physical reasoning, though wanting the ma-

thematical knowledge requisite for treating these subjects on the higher scientific plane to which their full theoretical development more properly belongs. It was well remarked by Dr. Lardner, that "the phenomena of heat all admit of being explained without the aid of abstruse reasoning, technical language, or mathematical symbols. The subject abounds in examples of the most felicitous processes of induction, from which the general reader may obtain a view of that beautiful logic, the light of which Bacon first let in on the obscurity in which he found physics involved." I may truly say that after the perusal of your book I understand the subject better than I did before; but still some of my doubts are unresolved, as you will perceive from the following statements.

Although a fixed equivalence undoubtedly exists between the heat generated by friction and the energy expended in producing the friction, *experiment proves* that elastic fluids suffer no diminution of heat in the act of producing work, but only a fall of temperature. I have long perceived the difficulties of admitting this hypothesis, as it involves the apparent paradox of a continual investment of energy in the production of molecular motion, of the reconversion of which back again into energy no account can be given. But the results of numerous and varied experiments leave me no alternative, and I must repeat my conviction that heat *runs down* but does not disappear in the production of work. Even admitting that quantitative heat disappears in the process, still many reasons might be given for doubting that any fixed equivalence exists between the heat disappearing and the work done; but for the present I will confine myself to the simple consideration that, with the disappearance of equal quantities of heat in the expansion of air of constant initial density, different quantities of work should be produced according to the mode of conducting the process.

Imagine a cylinder of 2 cubic feet capacity to contain 1 cubic foot of air of atmospheric pressure at the temperature of  $32^{\circ}$  F. confined by a piston resting at half stroke, supposed to be without weight or friction. Let the piston be fixed in this position, and heat applied to the air until its temperature is raised  $490^{\circ}$  above the initial point, and let the quantity of heat required to produce this effect be represented by 100. In this process the air is simply heated, and although statical tension is developed, no dynamical effect is produced, it is merely a preparation for work—a transfer of heat from one substance to another if the heat is taken either ready made, or produced chemically, or a change of some form of energy into heat by friction; but, as long as no work is done by the least air, things are considered to remain in a state of balance or equilibrium.

Suppose the air to be again at  $32^{\circ}$ , and the piston free to move when any disturbing cause should be applied. If heat be gradually communicated to the confined air it will expand, raising the piston and with it the superincumbent column of the atmosphere, which may be taken in round numbers at 15 lbs. per square inch. When 490 degrees of temperature have been given to the confined air, its volume will be doubled, and if the piston be 1 square foot area, it will have moved upwards through 1 foot; and it would be said that in its ascent it had performed the work of  $144 \text{ square inches} \times 15 \text{ lbs.} = 2160 \text{ lbs. raised 1 foot.}$  In this process 1 cubic foot of air has been heated 490 degrees, as in the former case, but the circumstances are different. In the former case the air was confined to constant volume, and tension was induced, but no work was done. In the present case the air was free to expand, no tension was induced, and work was performed; and as heat was the only energy employed in the operation, we should naturally expect an excess of heat to be used equivalent to the work done. In fact we find that 142 of heat has been thrown into the air in this case, while to heat the air to the same degree under constant volume 100 sufficed; it is therefore said that the difference, represented by 42, has done the work.

The work assumed to be done in the case under consideration, by raising the column of the atmosphere resting on the piston, is not available, it cannot be detached from the apparatus—a circumstance which might induce doubt as to its reality. If it possesses the entity which it appears to have, it should not be impossible to detach it from the matrix where it exists, and to exhibit it in some other form, thus removing the suspicion of imperceived coefficients acting in the phenomena. By altering the circumstances, the work which exists as potential energy in the raised atmospheric column ought, if all real, to be convertible into an equivalent of actual energy, or work done in another shape. By withdrawing the heat from the air in the cylinder, the piston will descend with a power corresponding to the difference of pressure between the total atmospheric pressure outside and the diminished pressure inside; and this power will be equal to half the atmospheric pressure at the beginning of the piston's stroke, diminishing to nothing at the end, when the confined air will, as at first, occupy 1 cubic foot at atmospheric pressure, and  $32^{\circ}$  temperature. The amount of power obtained in this operation will be less than a quarter of an atmosphere on the area of the piston moved through 1 foot. For simplicity of reasoning we may suppose it to be one quarter of an atmosphere.

In the working of a theoretically perfect thermic engine, *all*



the heat consumed, or which permanently disappears, is supposed to be converted into work in the proportion of Joule's equivalent of 772 foot-pounds for every unit of heat, neither more nor less. By means of a *regenerator* (as used in Stirling's and Ericsson's air-engines) of theoretically perfect action, all the heat withdrawn from the air as above described could be stored up in such a way as to be transferred back again to the same air under the same circumstances as before, and the operation could be repeated over and over again indefinitely, with the production of available work, provided that at each stroke of the piston a fresh quantity of heat represented by 42 be thrown into the apparatus. The heat consumed by 1000 strokes of the piston would be 1000 times 42, and the work produced would be 1000 quarters of an atmosphere on the area of piston raised 1 foot.

In the former case 1000 repetitions of the operation, with fresh air and fresh heat each time, would equally consume 1000 times 42 of heat, the 100 of heat remaining unchanged at the end of each operation; but if it is assumed that in each cycle of heating and cooling, expanding and contracting the confined air, work is actually done equal to raising the atmospheric column on the piston through 1 foot (*which is the apparent effect*), we must allow that the work done by 1000 repetitions of the operation would be 1000 atmospheres on the piston raised 1 foot. How is this apparent discrepancy to be accounted for?

Another serious difficulty in the dynamical theory is the question of the specific heat of air. Regnault's experiments appear to prove that the specific heat of air is constant at different densities. This result is in accordance with the dynamical theory, and in fact the theory could not stand unless this assumption were admitted. My own experiments have given very different results; but as it might be thought presumptuous to place these experiments in opposition to those of such a high authority as Regnault, I beg to submit the following reasoning to candid scrutiny.

Imagine a vacuum to be made in a cylinder by drawing up its piston by a descending weight, in which operation the work done by the descent of the weight gives an equivalent amount of potential energy in the tendency possessed by the atmospheric column raised by the piston to fall back into the vacuum; and so far things are in a state of equilibrium. Imagine further a second cylinder, twice the size of the former one, full of atmospheric-pressure air, which should be compressed into half its original volume by some exterior force applied, allowing the developed heat to pass out during the compression. In this operation work has been expended, and heat has been produced, which heat, according to the dynamical theory, would be consi-



dered equivalent to the work expended, as by allowing the air to expand back under moderated resistance, restoring to it the heat it had lost, the work done in the compression would be recovered. Now suppose a communication to be made between the two cylinders—the air will rush into the vacuum, and as the contents of the two vessels now filled by the expanded air are equal to the volume which the air originally occupied, it is evident that the pressure will also be equal to what it was before the compression, namely atmospheric pressure. It is clear that no power can be obtained from the expanded air, as it is in equilibrium with the surrounding atmosphere; but the energy which might have been availed of by allowing the atmosphere to fall into the vacuum, and thus produce work, has disappeared. It is impossible to conceive of the annihilation of energy, and we must therefore suppose that it has taken some other shape in the confined air. The dynamical theory does not allow the existence of energy in elastic fluids under any other form than that of molecular motion or heat, consequently there should be more heat in the expanded air; but as the experiments of Gay-Lussac and Joule have proved that in the expansion of air into a vacuum the temperature of the mass remains constant, we are forced to admit that the specific heat of the expanded air is greater than that of the air in a compressed state: in other words, the susceptibility of air to the influence of heat as affecting the thermometer increases with increase of density.

According to the dynamical theory, a common non-condensing steam-engine, working without expansion, utilizes only about  $\frac{1}{40}$  of the theoretical power of the steam. By the addition of a condenser this effect is doubled, or the engine gives about  $\frac{1}{20}$  of useful effect. Such were the deductions published by Regnault, conveying to abstract theorists the idea that the steam-engine is one of the most imperfect of prime movers. Some of the results obtained by M. Hirn from the practical working of large steam-engines were as high as  $\frac{1}{6}$ , none lower than  $\frac{1}{10}$ ; and  $\frac{1}{8}$  may be taken as a possible degree of practical efficiency—better certainly than Regnault's deductions, but still showing that the steam-engine under the most favourable circumstances is so far from perfection as to throw away  $\frac{7}{8}$  of the theoretical power of the steam. However, the marked discrepancy between the theoretical deductions and the practical results showed that something was wrong in the theoretical calculation; and the excess of work obtained in Hirn's results has been accounted for by supposing that some of the steam condenses in the act of expansion in the working of the engine, and that this condensation represents the heat converted into work beyond the theoretical calculation which supposed all the steam to pass into the condenser in the vaporous

state. This explanation involves the idea that steam at different densities contains a variable quantity of heat in proportion to the work it can perform ; and this inference obtains some slight support from the results of experiment, showing that the total heat of steam increases with increase of density, but by no means enough to account for the greater amount of work it can perform by expansion.

One of M. Hirn's experiments with an engine cutting off the steam at  $\frac{1}{3}$  of the stroke showed at the same time a production of work and a creation of heat. In another of his experiments steam was thrown into a sheet-iron vessel immersed in cold water, and it was found that the heat thrown out by the condensing steam was more than that due to its constitution as deduced from calculations of acknowledged correctness. Moreover the fact that steam becomes superheated in the act of free tumultuous expansion is not disputed, though it involves the idea of some other form of energy existing in the steam besides molecular motion. The dynamical theory necessitates the assumption that steam is condensed during expansion in the production of work, as theoretically shown by Clausius and Rankine ; and another of Hirn's experiments would seem to corroborate this assumption. A copper tube upwards of 6 feet long and 6 inches diameter, with strong glass ends, was put in communication, by means of stopcocks, with a steam-boiler at one end and with the atmosphere at the other. The tube full of high-pressure steam was perfectly transparent ; but the moment the cock communicating with the atmosphere was opened, and part of the steam rushing out allowed the remainder to expand, a cloud was formed in the tube : the steam became quite opaque, showing, as it is supposed, the condensation of the steam which should accompany expansion.

On this experiment, I would remark that a film of moisture must exist on the interior surface of the tube, though at the same time the contained mass of steam may be dry, or free from particles of water mechanically suspended in it. The temperature of the copper must be somewhat lower than that of the high-pressure steam, but much higher than the temperature of the steam at low pressure ; consequently, on the sudden lowering of pressure in the tube when the steam is allowed to blow off, an instantaneous "flashing" into steam of the water on the now comparatively hot metal must ensue, with a large amount of *priming*, or dispersion of the water in the shape of fine spray, probably quite sufficient to account for the opacity observed. The experiment should be made with the tube enclosed in a case with glass heads, filled with steam of the same temperature as the steam inside, to prevent the deposition of moisture on the interior surface of the tube. This precaution would in a degree

obviate a serious source of error, though still it should be borne in mind that glass has an attractive power for moisture which prevents its free evaporation at temperatures considerably higher than the regular boiling-point under any given pressure.

I must be allowed to express my opinion that the experiments of M. Hirn are of great practical value, and I submit that the importance of the facts which they furnish is not depreciated by the circumstance that the experimenter, apparently borne down by scientific authority, recently presented to the Academy of Sciences an interpretation of his results in accordance with the dynamical theory. It should also be borne in mind that the results of Hirn's experiments coincide in the main with those of Séguin made more than twenty-five years ago, when his reasoning on the mode of action of the steam-engine pointed out the necessity of supposing a *conversion of heat into work*, of which, nevertheless, he could find no appreciable indication in the results of the numerous experiments made by him with this express purpose, and conducted with the attention and accuracy suggested by his great scientific knowledge and superior practical skill in thermo-dynamics as then known.

In my experiments above alluded to on the specific heat of air at different densities, I found that certain proportions of surface in the apparatus relative to the masses of air, their condition, and the times occupied by their passage through the calorimeter, were essential to correct results; and I venture to think that some concealed source of error must have existed in Regnault's experiments on this subject, undetected even by his extraordinary ability in similar researches.

I would also repeat that my long practical acquaintance with the general phenomena of steam, as well as the results of direct experiment, have tended to convince me that the total heat of steam is a constant quantity. I believe that this important point is again engaging the attention of physicists; and it may be well to remark that in the condensation of high-pressure steam there is a tendency to produce heat from the effects of tumultuous expansion, which may seriously affect the results, showing, as in some of Hirn's experiments, more heat than is due to the constitution of the steam. In the cooling of gases under ordinary circumstances there is no change of state, and the contraction of the volume should result from a gradual shrinking of the sphere of action of each particle in the mass, a process which suggests no idea of tumultuous motion or percussive action of the particles. In saturated vapours each particle must possess the precise specific amount of heat requisite for its existence in the vaporous state: if heat be added, the vapour becomes a gas; if heat be withdrawn, some of the particles must collapse into



liquid. This collapse should evidently be sudden, and each particle in assuming the liquid state should, for the moment, leave an almost perfect vacuum in the space it occupied in contact with the cold walls of the condenser. The void is instantly occupied by a fresh particle of vapour, which in its turn collapses, and so the process continues as long as vapour remains to be condensed. It may be conceived that the particles of vapour thus rapidly precipitated into a vacuous space, bounded on the outside by the hard metallic walls of the condenser, must strike with violence against these surfaces, and generate heat by this molecular battering.

It may be replied that the internal work done by the remaining particles of vapour in forcing their fellows down the precipice of the vacuum should cause an amount of chilling of the mass by an actual disappearance of heat equal to that generated by the percussive action of the particles against the sides of the condenser. To this I would answer that, until some good account can be given of the actual excess of work-producing power which high-pressure steam is allowed to possess as compared with steam of low pressure, weight for weight, and which is *apparently lost* by the quiet condensation of the steam in the vessel which contains it, we may suppose some unacknowledged form of energy to exist in the steam distinct from mere molecular motion, though always acting in cooperation with it, and more than sufficient to cause the violent fall of the vapour particles against the walls of the condenser, thus generating heat in excess of what is fairly due to the constitution of the steam.

The very nature of steam suggests the idea that, as far as quantitative heat is concerned, each individual particle should have a fixed specific temperature, the general temperature of any given mass resulting simply from the number of particles in a given space, or, in other words, the distance of the particles from each other, and consequently the amplitude of their molecular motions. Thus it is certainly reasonable to conceive of a fall of temperature in expanding steam without any idea of absolute loss of heat. However this may be, I would beg to impress on the attention of physicists who may be making further experimental researches on this interesting point, the importance of guarding against all probable causes of error which can be foreseen, keeping in mind the results obtained by Séguin and Hirn, and also the experiments of Siemens, in which high-pressure steam seemed to *superheat itself* by tumultuous expansion.

I remain, Sir,

Your most obedient Servant,

JOSEPH GILL.

Palermo, January 4, 1864.



XVII. *Principle of the Equilibrium of Polyhedral Frames.*

By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS.L.&amp;E.\*

THE following theorem is the extension to polyhedral frames of a principle which is proved for polygonal frames in 'A Manual of Applied Mechanics,' art. 150.

THEOREM.—If planes diverging from a point or line be drawn normal to the lines of resistance of the bars of a polyhedral frame, then the faces of a polyhedron whose edges lie in those diverging planes (in such a manner that those faces, together with the diverging planes which contain their edges, form a set of contiguous diverging pyramids or wedges) will represent, and be normal to, a system of forces which, being applied to the summits of the polyhedral frame, will balance each other—each such force being applied to the summit of meeting of the bars whose lines of resistance are normal to the set of diverging planes that enclose that face of the polyhedron of forces which represents and is normal to the force in question. Also, the areas of the diverging planes will represent the stresses along the bars to whose lines of resistance they are respectively normal.

It is obvious that the polyhedron of forces and the polyhedral frame are reciprocally related as follows: their numbers of edges are equal, and their corresponding pairs of edges perpendicular to each other; and the number of faces in each polyhedron is equal to the number of summits in the other.

Glasgow, January 9, 1864.

XVIII. *On the Theory of the Velocity of Sound.*

By Professor CHALLIS, M.A., F.R.S., F.R.A.S.\*

THE "Note" of Professor Tyndall "On the Velocity of Sound" in the Number of the Philosophical Magazine for last November, and the reference therein made to Dulong's experiments, have led me to see that the principles I have applied to this question admit of an extension which had not previously occurred to me. For this reason, and because I am desirous of making a few remarks on the notice taken of my researches by Prof. Le Conte in the article inserted in the January Number, I now revert to the subject.

The theoretical value of the velocity of sound which I have obtained, agreeing very closely with the observed value, is a purely mathematical deduction, on hydrodynamical principles, from the hypotheses that the medium is a perfect fluid, and that

\* Communicated by the Author.

its pressure ( $p$ ) varies as the density ( $\rho$ ) under all circumstances, whether of rest or of motion. If  $p_0$  and  $\rho_0$  be given corresponding values of the pressure and the density, we shall always have,

according to these hypotheses,  $p = \frac{p_0}{\rho_0} \cdot \rho$ . Also the velocity of propagation, determined by the investigation just referred to, is

$\kappa \sqrt{\frac{p_0}{\rho_0}}$ ,  $\kappa$  being a certain numerical constant, the same for all

perfect fluids. Hence if experiments be made for ascertaining the velocity of sound in different fluids in such a manner that  $p_0$  is the same for all, the different velocities should vary inversely as the square root of  $\rho_0$ . That condition was satisfied in the above-mentioned experiments of Dulong, which are contained in vol. xli. of the *Annales de Chimie et de Physique*. The results given in p. 150 for seven gases, show that the velocities conform to the above law in the instances of atmospheric air, oxygen, hydrogen, and oxide of carbon; but that the velocities deduced according to the law from that in atmospheric air for carbonic acid, oxide of nitrogen, and olefiant gas exceed the experimental values by  $8^m.1$ ,  $7^m.6$ , and  $22^m.2$  respectively. The physical reason for these differences I now proceed to investigate.

The following explanation is based on physical principles which I have frequently enunciated in this Magazine, conjoined with certain experimental results recently obtained by Professor Tyndall. I suppose an elastic fluid to consist of inert spherical atoms of constant magnitude, each of which, by means of the reflexion of ætherial undulations from its surface, becomes a centre of repulsive force, the undulations being of necessity such that by their dynamical action they keep the atoms asunder. The aggregate effect of this action of *heat-undulations* is to produce a pressure of the fluid proportional to its density, as I have endeavoured to show by the solution of Problem II. at the end of the "Theory of Molecular Forces" contained in the Philosophical Magazine for February 1860. Now if any portion of the fluid be compressed within a smaller space, and the repulsive force from each atom (which for brevity I shall call its *specific heat*) be supposed to remain the same as before the compression, the increase of pressure of the fluid will be entirely due to the greater proximity of the atoms to each other, the atomic repulsion increasing with the diminution of distance. If the two states of compression be separated by a considerable interval of time, there is no difficulty in admitting that the specific heat of each atom is the same in both, because its amount is determined by the general temperature of surrounding substances. But would the case be the same during the rapid changes of density which occur in the vibrations of the fluid? The answer to this ques-

tion appears to me to depend on a property of elastic fluids relative to heat which has been experimentally ascertained by Prof. Tyndall. The experiments show that certain fluids, as atmospheric air and simple gases, are, like rock-salt, permeable to heat-undulations, just as transparent substances are permeable to light-undulations. Consequently in these fluids the addition or abstraction of heat-undulations is not accompanied by such disturbance, or breaking up, of the undulations as would produce change of temperature and radiation. Now if we may suppose that the heat-undulations from the atoms of these same fluids always remain in like manner undisturbed when the atoms are caused to approach each other by compression, or to recede from each other by dilatation, we shall have reason for concluding that the sudden changes of density in vibrations of the fluid do not give rise to changes of *temperature* and elasticity, but only produce the changes of caloric repulsion to which are due the changes of *pressure*. By this process of reasoning we are evidently conducted to the inference that the actual velocity of sound should be mathematically deducible, at least very approximately, on the supposition that the elasticity of the fluid is the same in a state of vibration as when it is at rest. I have, in fact, obtained in this way a velocity very little less than that given by direct observation.

It is, however, an ascertained fact that sudden condensations of the air develope heat, and sudden rarefactions abstract heat. A physical explanation of these effects may be given on the following principles. When there is an equilibrium of temperature, as much heat is received within a given space as issues from it, whatever be the density of the substances in the space. Hence as a sudden condensation, by bringing into a given space an additional number of atoms, suddenly increases the number of centres of heat-undulations of given specific intensity, the equilibrium of temperature is momentarily destroyed. A like effect is produced by a sudden rarefaction. But while such effects are being produced, the dispersion, by radiation, of the excess or defect of heat to surrounding substances, is continually tending to restore the equilibrium. When it is considered that this radiation traverses space much more rapidly than the vibrations of the elastic medium, and that consequently simultaneous effects of the excess and the defect of heat may at any given point very nearly neutralize each other, and also that *time* is always required to produce changes of temperature, there seems no good reason to conclude that any but inconsiderable changes of temperature and elasticity result from this alternate generation and absorption of heat. The changes, for instance, in unconfined air cannot be comparable with those which would be caused by the



sudden condensation or rarefaction of air contained in space limited by partitions which prevent the dispersion of the generated heat and cold into the surrounding space. The tendency to such dispersion is proved in the common experiment by which German tinder is ignited. It may, however, be admitted that the effects of the development of heat in the condensed part of a wave, and of cold in the rarefied part, do not completely destroy each other, and that there is a residual gradation of temperature and elasticity from point to point of the wave, sufficient to account for a slight augmentation of the velocity of propagation. If my mathematical argument be good, this is all that we have to account for.

But if an elastic fluid does not possess the property of transmitting heat-undulations without their undergoing partial degradation and dispersion, the foregoing conclusion is no longer true. The Theory of Molecular Forces on which the conclusion rests, assumes that the medium is perfectly transmittent with respect to heat. But Dr. Tyndall's experiments show that while this is the case very approximately in simple gases and atmospheric air, other gases, as especially olefiant gas, do not possess the same property. In these, accession of heat was observed to be accompanied by radiation, and abstraction of heat by absorption, under circumstances in which such effects do not take place in atmospheric air. Consequently in such gases the changes of caloric action produced by forcible alteration of the density of the fluid have not their full dynamic effect on the atoms in producing alteration of pressure, being partly expended in the observed radiations and absorptions. This conversion of the undulations is analogous to that which light-undulations undergo when transmitted through a partially transparent medium. Hence, so far as this cause of disturbance operates, the fluid is imperfectly elastic, and transmits sound-waves with a velocity *less* than that which accords with the relation between the pressure and the density in the quiescent state. It is particularly to be noticed that the alteration of that relation here considered altogether depends on a transient disturbance of the statical condition of heat, produced by a *sudden* alteration of density, and that the law of Mariotte holds good as soon as the equilibrium is restored.

But it may be urged that the property of radiation and absorption of heat in olefiant gas has itself the effect of altering the velocity of propagation. While this may be admitted, the following reasons may be given for concluding that the effect is very small. According to the views explained above, the radiation of heat and cold by olefiant gas when in a state of rapid vibration, is an effect of the same kind as that which is pro-

duced by suddenly increasing and suddenly diminishing the number of atoms in a given space, and which takes place as well in olefiant gas as in atmospheric air. Thus, although both kinds of radiation are brought into play in sudden changes of density of olefiant gas, the rate of propagation may not be thereby sensibly altered, because all the reasons above adduced for concluding that the latter radiation produces but little alteration are applicable to the former. So far as they have effect, they *increase* the velocity of propagation by augmenting the gradations of temperature and elasticity. There remains, then, only the above-mentioned effect of the imperfect elasticity of the gas in vibration, to which may reasonably be ascribed such excesses of the theoretical above the observed velocities as those in the instances of carbonic acid, oxide of nitrogen, and olefiant gas, which were adduced towards the beginning of this communication.

The foregoing explanation, which rests entirely on the new principles of theoretical physics which I have proposed in various previous articles, is one among many explanations of physical phenomena which I have given on the same principles, and adds to the cumulative evidence of the truth of the fundamental hypotheses.

With respect to Prof. Le Conte's paper "On Laplace's Correction of the Theoretical Velocity of Sound," I beg in the first place to remark that any decision respecting the validity of that correction necessarily involves a full consideration of the mathematical reasoning by which the theoretical velocity was obtained, because the correction implies a previous mathematical determination. Now the mathematical investigation of the motion of fluids was left by Lagrange in a very imperfect state; and no important addition was made by Laplace. The theoretical velocity which they accepted has therefore no right to be considered final. By an extension of the principles on which the mathematical reasoning is based, I first succeeded in showing that the velocity of propagation is greater than that which had been generally adopted, and subsequently found for air a numerical value closely agreeing with the observed velocity of sound. This result, being obtained by reasoning from admitted premises, ought certainly to be capable of disproof if it is not true. I will also concede that the mathematical reasoning will be proved to be untrue, if the development of heat and cold by the sudden condensation and rarefaction of the air in vibration be, as Prof. Le Conte contends, a *vera causa*, accounting for an increment of velocity equal to one-sixth of the observed value. But this is a point of much difficulty, on which I find that French writers speak with a degree of caution that has not been generally imi-

tated. Thus Poisson, in his "Memoir on the Theory of Sound," says that the difference between calculation and observation may be attributed solely to development of heat, "if no objection can be raised against the analysis by which the velocity of sound was deduced." Then after *assuming* that air in vibration undergoes changes of temperature proportional to the changes of density, and finding that if a compression equal to  $\frac{1}{116}$  be accompanied by an increase of 1 degree of temperature the velocity of sound will be accounted for on that assumption, he admits that such an increment of temperature in aerial waves "cannot be verified by any direct experiment" (*Journal de l'Ecole Polytechnique*, vol. vii. p. 326 and p. 363). Dulong, in the memoir already cited, writes thus (p. 128): "Poisson arrived at the same theorem [that of Laplace relative to the ratio of the specific heats] by a calculation more direct and wholly unencumbered by the improbable hypotheses which the author of the *Mécanique Céleste* had adopted respecting the mode of existence of heat in elastic fluids." On turning to the memoirs of Poisson here referred to (*Ann. de Chim. et de Phys.* vol. xxiii. p. 337, and *Conn. des Temps*, 1826, p. 257), it will be found that he dispenses with Laplace's *a priori* considerations by making the assumption just mentioned, which is accordingly the foundation of his theory. In a note in the *Conn. des Temps* (p. 269) he adds, for the sake of further explanation, that "the actual temperature of a slice of fluid in movement subsists only for an instant, and only extends through an infinitely small thickness; but that does not prevent our conceiving of that temperature and its measure." A physical circumstance which is only "conceivable" is, I submit, not a *vera causa*. In short, the objection I have constantly made to Laplace's correction is, that there are absolutely no experimental data for calculating the effect of the development of heat in aerial waves, and that experiments on air in confined spaces do not furnish such data. From not taking this distinction into account, Prof. Le Conte has not correctly represented my views where he says that I contend that "experiment proves the development of heat by condensation only when the fluid is confined within narrow limits." I allow that heat is developed by condensation, whether the fluid be confined or not, but deny that the effect of the developed heat in changing the temperature is the same under the two circumstances.

It must, however, be conceded that full weight is to be given to the evidence adduced by Prof. Le Conte of the close agreement of the theoretical velocity of sound with observation after the application of the Laplacian correction. If the assumed *modus operandi* were made matter of demonstration, the evidence would be irresistible. Till this be done, the accordance of my



mathematical result with observation is entitled to at least equal weight. The coincidence of the numerical result obtained from experiments made for ascertaining the mechanical equivalent of heat, adds nothing to the evidence, because these experiments are apparently only another form of those by which the ratio of the specific heats is determined.

I take this occasion to advert to a result respecting the velocity of sound which I have given at the end of an article in the *Philosophical Magazine* for April 1862. I have there found that the rate of propagation depends, in some degree, on the loudness of the sound. But after taking into account that I had previously demonstrated the component character of the waves of elastic media, it appeared to be a necessary consequence that the greater or less magnitude of waves depends only on the greater or less number of the components, and hence that the velocity  $v$  in the cited formula is the maximum velocity in an uncompounded wave. Now as there is no antecedent reason for supposing that the maximum velocity is greater for one such wave than for another, the right conclusion seems to be that all the component waves, and therefore that all waves, great and small, are propagated with the same velocity. It may, however, happen that of two disturbances, made at the same time and at the same distance from a given position, one agitates the air at the first moment through a greater extent than the other; in which case the effect of the former would reach the given position by propagation sooner than that of the other.

Cambridge, January 20, 1864.

XIX. *Reply to some Remarks of Dr. Le Conte in his paper on the Problem of the Velocity of Sound. By S. EARNSHAW, M.A., Sheffield\*.*

I AM not disposed to enter upon any general review of Dr. Le Conte's paper printed in your Magazine of this month, having in fact no leisure for the task; but as some portion of it is devoted to a review of three papers of mine which were printed in your Magazine in the year 1860, it seems to be necessary that I should take notice of his remarks and reply to them as far as they concern myself.

In page 8 of his communication, having given an abstract of my results, he asks in disparagement of them, "Is it admissible to assume *the law of force*, according to which the molecules of the atmosphere act on each other, *to be that of the fourth power of the inverse distance*? The only reason given for this assump-

\* Communicated by the Author.

tion is," &c. To this I answer that Dr. Le Conte has quite misunderstood my investigations. No such assumption as this which he here calls in question is made in them. There is no difficulty, however, in seeing what is the origin of his mistake. He has mistaken the *differential coefficient* of the law of molecular action for the *law itself*. The law assumed was that of the inverse *third power* of the distance; and for such an assumption there is very good authority, besides that which is alleged in the investigation where the assumption is made. For if Dr. Le Conte will turn to the Cambridge Philosophical Transactions, vol. vii. part 1, he will there find a paper, entitled "On the Nature of the Molecular Forces which regulate the constitution of the Luminiferous Æther," and in that he will find it proved by strict mathematical reasoning, that if the particles of an elastic medium act on each other according to an inverse power of their distances, *that power must be greater than 2*; and as it is hardly accordant with the simplicity of nature to suppose the power fractional, the simplest and therefore the most likely power is 3. My investigation in your Magazine led me to the same result, though by a process quite distinct from that employed in the Cambridge Transactions. I hope, therefore, that after reading this statement Dr. Le Conte will not think the assumption I have made (not that which he erroneously ascribes to me) so extremely improbable as to justify him in condemning my theory on its account.

He asks another question also in disparagement of my theory, a question which no person acquainted with the integral calculus would think of putting forward as any objection to my results: "Does not the fact that the analytical processes lead to *two* entirely distinct *types of waves* indicate that there must be some error in the assumptions of the mathematician?" To this I answer that the equations of motion lead to exponential integrals, which it is well known take the form of circular integrals under certain relations of the parameters. Thus there are of necessity two possible forms of the integrals, the exponential and the circular, and each of these has its own physical interpretation. Thus, then, it appears that there are two possible types of waves, which for distinction's sake I denominated the *violent* and the *gentle*. The objection which Dr. Le Conte raises to the duality of possible wave-types can only be owing to his want of acquaintance with mathematical processes, and the physical interpretation of mathematical formulæ. This explanation involves also a sufficient answer to another disparaging query which Dr. Le Conte asks: "Is it not almost a *physical impossibility* that the *same elastic medium*" &c. The obvious reply to this is, that the mathematical investigations show that *it is not* "almost a physical impossibility" that an elastic medium should transmit waves of

different types with different velocities. On the contrary, they show that the difference of velocities is a strictly legitimate and necessary consequence of the difference between the wave-types, one being exponential and the other circular; and it is in fact exactly analogous to this—viz. that  $\frac{1}{2}(e^x + e^{-x})$  can never be less than 1, and  $\frac{1}{2}(e^{x\sqrt{-1}} + e^{-x\sqrt{-1}})$  never greater than 1, whatever value be given to  $x$ . It arises from this that the velocities of transmission of one type of wave range from  $V$  to infinity, and those of the other type from  $V$  to 0. I hope that after this explanation Dr. Le Conte will not continue to think that my “results set at nought all our physical conceptions,” and will agree with me in believing that they are very natural consequences of the physical hypotheses from which they follow, and open to us a new field of wonder and admiration in the world of sound. He seems to have formed an opinion, based on his notion of what an almost physical impossibility is, that my results belong to a class of *mathematical fictions*. I can assure him that they are strictly deduced from the original physical assumptions; and therefore if there is any fiction in the matter it must lie, not in the mathematical results, but in the physical hypotheses from which they are deduced; and those hypotheses are—

1. That the atmosphere is a medium of separate molecules; and
2. That the molecular force varies according to the inverse *third* power of the distance. Perhaps Dr. Le Conte will be now satisfied that there is *not much fiction* in the matter.

But as I supported my results, so far as they differ from previously received opinions, by reference to certain physical facts, Dr. Le Conte next proceeds to discredit my facts. He refers to the velocity with which the sound of thunder-claps is transmitted, which according to my theory ought to be greater than that of ordinary sounds. He disposes of my facts about thunder-claps by remarking that flashes of lightning are sometimes several miles in length, and that “the observer estimates one element for determining the velocity, viz. the *distance*, under the assumption that *the sound is generated at the point struck by the electricity*.” Now I will grant that an inexperienced and ignorant observer might do this; but my facts were furnished to me by Professor C. Montigny, who knew a great deal better what he was doing than to fall into such a learner’s mistake. Besides, it is quite plain that Dr. Le Conte has never considered the nature of the evidence. Let us take one of Prof. Montigny’s facts. There were two observers, who were about equidistant from each other and from the point struck. That distance was such that, according to the common theory, it would take sound about 15 seconds to go from one to the other. Now Dr. Le Conte shall have a flash of lightning as long as he likes, and I ask him to



show how it could strike one corner of the equilateral triangle so as to be heard at each of the other corners (where the observers stood) in 2 seconds. Surely he will be able to see that it is a mathematical impossibility that the flash should fall in any way whatever so as to be heard by each of the two observers, according to the common theory, in less than  $7\frac{1}{2}$  seconds. But it was heard by each of them in 2 seconds. Here then is a *proof* that the sound was transmitted in this case *nearly four times as fast* as the common theory allows. And yet with this unanswerable fact before him, supported by other confirmatory observations by Prof. Montigny, Dr. Le Conte asserts that I have “failed to produce a single unexceptionable fact, or a single satisfactory observation, in verification of my theoretical deductions.” Such assertions as this are easily made, even in the presence of plain evidence to the contrary; but they should never be introduced into inquiries after scientific truth.

And again, with regard to Captain Parry’s record of the word “fire” having been heard after the report of the gun in his experiments on sound. How does Dr. Le Conte dispose of that fact? There are persons now living who were present at the experiment and noticed the fact. There was no room for doubtful opinions. The fact is of such a nature that they could not be mistaken. They testify that they distinctly heard the word “fire” after the sound of the gun’s report had passed them. But Dr. Le Conte gets rid of this well-authenticated fact in a very simple and summary way, a way by which any other unpleasant fact may also be got rid of. He tells us that “the records of the most trustworthy experimentalists concur in establishing the general fact *that all sounds travel at the same rate.*” Captain Parry’s officers we see, then, were not trustworthy experimentalists. But I deny the truth of the assertion made here by Dr. Le Conte. For let it be remembered that the question at issue is not whether all gentle sounds (*i. e.* sounds of the musical type) travel at the same rate, but whether a violent sound travels at the same rate as a musical sound; and I deny that experimentalists concur in establishing this to be a fact, as Dr. Le Conte asserts they do. That all *musical* sounds travel at the same rate has been most satisfactorily proved; and this result agrees accurately with my theory, and confirms it. But with regard to the comparative velocities with which violent and gentle sounds travel, any person who will turn to Sir J. Herschel’s Table of experimental results, given in art. 16 of his ‘Treatise on Sound,’ will find there abundant cause for suspecting that the velocity of sound depends in some way on the mode of its genesis. Captain Parry’s fact still therefore stands an obstinate witness with Prof. Montigny’s facts in favour of the truth of my

theory ; and Dr. Le Conte's assertion that I have not produced a single fact confirmatory of my theory is disposed of. It is not to be expected that I should be able to lay my hands on many, or even on any, recorded observations made previous to the publication of my papers on the subject, because previous to that time experimentalists were under the conviction that all sounds travel at the same rate. This had been rigorously proved in the case of sounds of the musical type ; and not being aware of the duality of types, they took it for granted that what was true of musical sounds was true of all sounds. They did not look for any difference of velocities, therefore ; and the difference would have had to be *very* considerable before they would take notice of it so as to make a record of it.

Dr. Le Conte represents me as saying that "sound-waves embracing velocities from  $\frac{1}{2}V$  to infinity are within the limits of experimental verification." I simply deny that I have said so. The spirit of my papers is directly opposed to such a statement. Contrary to his assertion, I have carefully said that all audible sounds travel with velocities equal to  $V$  and upwards towards infinity ; and Dr. Le Conte's inference, that I ought to be required to verify these velocities (from  $V$  to  $\frac{1}{2}V$ ) experimentally, falls, like his other assertions, to the ground. Both Prof. Montigny and Captain Parry have testified that there are sounds which travel with velocities not equal to, and exceeding, the velocity of common sounds, which is all that I have asserted from my investigations.

I should like to terminate my letter at this point ; but there is one other remark of Dr. Le Conte's which I must not leave unanswered, because he evidently thinks he has put me on the horns of a dilemma from which there is no escape for me. He tells me that my theory takes no cognizance of the development of heat, which I admit to be a *vera causa*. He is surprised that I should have any confidence in a formula obtained without taking any account of a *vera causa*. My answer to this is as simple as even Dr. Le Conte can require : I *did take account of this vera causa* in my investigations. But Dr. Le Conte will answer, "You have said nothing about taking it into account in your investigations." True, I said nothing about it, but I took it into account for all that. How ? Let me explain. The *vera causa* amounts to this—that the whole quantity of heat which was distributed among a certain set of atmospheric particles before the sound-wave reached them remains among the same set of particles during the whole time of the wave's passage past them. In other words, not any particle loses or gains *heat* (it may lose or gain *temperature*) during its motion. Now heat is the cause of the repulsive action among the particles, and is

represented by the quantity of *absolute molecular force*. The mathematician who has to deal with forces, takes account of the quantity of heat existing among the particles when he assigns a magnitude to his absolute molecular forces; and if these forces continue unchanged through his investigations, this unchangeableness is the mathematical expression of the physical fact that no heat escapes during the compression, and that none is gained during the dilatation that takes place in the passage of a wave. Now in my investigations the absolute molecular forces remain constant throughout; and consequently the physical hypothesis is perfectly taken into account that no change in the quantity of heat in a given molecule or element of the atmosphere takes place; which otherwise expressed, in the language of the physicist, is this: the heat developed during condensation, and the cold of rarefaction, are fully taken account of. Dr. Le Conte seems to have forgotten that, in speaking of gases, *heat* and *molecular force* are equivalent terms, the one belonging to the language of the physicist, and the other to that of the mathematician. Mathematicians, by adopting the hypothesis of the continuity of the aërial medium, had nothing to do with molecular force in their investigations, any further than as it might be contained in the further assumption of a relation between pressure and density. If they assumed *Boyle's* law, then in so doing they assumed that heat escaped by radiation as fast as it was developed by compression; but if they assumed *Laplace's* relation between pressure and density, then they in so doing assumed also that no heat was lost by radiation. But in the supposition of finite intervals between the molecules of the aërial medium, the mathematician, as a matter of course, supposes the absolute molecular forces constant throughout his whole investigation, and therefore in doing so takes perfect cognizance of *Laplace's* suggestion without having even so much as to mention it. The assumption of a medium constituted of separate particles includes *Laplace's* suggestion, and takes account of it without the necessity of knowing anything about it. And on this account it is that I said that, had mathematicians adopted the finite-interval theory, instead of the continuity theory, and worked out the integrals (as I have done) without introducing changes in the forms of their differential equations for the purpose of effecting approximate integrals, there never would have been any occasion for *Laplace's* suggestion. Theory would have furnished a velocity large enough without any additional suggestions.

Before concluding, I must say one word more. I am one of those who think that *some* heat does escape during the passage of a wave of condensation through a given element of the atmospheric medium. I find it difficult to believe that a given



element of the medium can be kept at an elevated temperature for the  $\frac{1}{20}$ th or  $\frac{1}{30}$ th of a second, during which it is within the limits of a sound-wave, without some heat being lost by radiation in that comparatively long time. If *some* heat is lost, then the finite-interval theory will give a velocity of sound which will be in excess of experiment, because that theory essentially supposes that *no* heat is lost. The velocity to which my investigations lead is therefore the superior limiting value of sound-velocity, and in practice must be reduced by an allowance made for the loss of heat—that is, for the loss of absolute molecular force which occurs during the motion. I look upon 1130 feet, the velocity as determined by my theory, as the extreme possible value of ordinary sound-velocity. It is probable the experimental value will be something less than this theoretical value, just in proportion as it is probable there is a loss of heat by radiation. And upon this point I may just remark that Dr. Le Conte thinks that “during the propagation of elastic impulses the condensations and rarefactions are necessarily momentary.” I am of a contrary opinion. They are not momentary; for they last during the whole time that it takes a wave to pass a given element of the medium. In the case of long waves this may be the  $\frac{1}{30}$ th or even the  $\frac{1}{10}$ th of a second, which is surely much larger than a moment, and quite long enough to allow heat to escape by radiation. It is certainly not the *momentariness* of the condensations which prevents the escape of heat by radiation. Nothing that Dr. Le Conte has written has changed my opinion of the state of the problem of sound. We want careful experiments on the comparative velocities of sounds due to different exciting causes, such as musical instruments, blows with a hammer on a hard substance, explosions, discharges of electricity, cannon, thunder, &c. We know but little yet of the properties of different types of sound.

Sheffield, January 16, 1864.

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XX. *A Reply to the Observations of Prof. Le Conte, M.D., respecting the adequacy of Laplace's suggestion for a correction in the Theory of Sound.* By PROFESSOR POTTER, A.M.\*

**I**N Dr. Le Conte's paper published in the Philosophical Magazine for January 1864, he says it appears to him that the obscurity which marks many of the discussions relating to the problem of sound arises from two distinct causes, “namely, *first*, from a *misconception of the physical theory of Laplace and that of Poisson, which is substantially the same*; and *secondly*, from the *difficulties and obscurities which invest the mathematical theory of*

\* Communicated by the Author.

partial differential equations in their applications to physical questions.”

Since Dr. Le Conte attributes misconception of the physical theory of M. Laplace's correction in the problem of sound to others, he of course challenges examination with respect to his own conception of it. Now the original suggestion of M. Laplace was made to M. Biot, who put it into what he considered a mathematical form\*. This solution, from the intimacy between them, very probably received the assent of Laplace before it was published, and would therefore be his first solution, and would embody his original views. It was several years later that M. Poisson showed the solution to be erroneous†, and gave the mathematical form of the expression for the elasticity of an elastic fluid in a state of vibratory motion which was required for the solution of the problem. After many years again (in 1816), Laplace adopts Poisson's solution‡ in another form, and finally, in the *Mécanique Céleste*, admits that they are the same solution, and claims them to have originated from his suggestion.

We have here two points to consider, namely, Laplace's suggestion to Biot, and Poisson's solution of the problem. With respect to Laplace's popular suggestion, the objections are unquestionably valid, that a consideration of cold developed in rarefactions might be applied to account for a smaller velocity than the Newtonian one, as reasonably as a development of heat in condensations to account for a greater one, and have never been answered. Dr. Le Conte, like others, diverts the argument to Poisson's solution, which is quite another matter.

M. Poisson found that, in order to obtain a strict mathematical solution, it was necessary to assume the formula

$$\sigma = \beta s$$

in the expression for the pressure ( $p$ ) in the disturbed fluid,

$$p = gmh(1 + s + \sigma);$$

and  $\sigma = \beta s^3$ ,  $\sigma = \beta s^5$ , &c., or the product of  $\beta$  with any other odd power of  $s$  would satisfy the conditions, but  $\sigma = \beta s$  would alone furnish the required solution. It was required also that the temperature of the gas should rise  $1^\circ$  Centigrade for a condensation  $\frac{1}{16}$ th part, and this also *simultaneously* with the condensation.

Now these were assumptions; and Dr. Le Conte says rightly, “in problems of this character, no deduction from analysis is worthy of confidence which does not admit of a rational physical

\* *Journal de Physique*, vol. lv., for 1802.

† *Journal de l'Ecole Polytechnique*, vol. vii. cahier 14 for 1807.

‡ *Annales de Chimie et de Physique*, vol. iii. p. 238.

*interpretation, capable of being tested by observation or experiment."* Experiments show that the temperature of air escaping from a greater to a less pressure does not change suddenly, but occupies a short interval of time, like the change of temperature in the high-pressure steam-jet\* at different distances from the aperture. This want of simultaneousness of the change of temperature is of itself fatal to Poisson's solution. Again, it was found by myself† that *near the aperture* in a jet of air the change of temperature varies *as the cube* of the rarefaction, and not as the first power, which is required in Poisson's solution, which consequently fails also on a second account. Dr. Le Conte's conception of Laplace's correction in the theory of sound, whether on the original suggestion, or in the discussion of Poisson, has thus no experimental confirmation, which he rightly states to be necessary.

The methods of finding the heat and cold developed in condensations and rarefactions by the air-thermometer, after the method of Clement and Desormes, which was used also by Gay-Lussac and Welter at the wish of Laplace, I have shown to be radically faulty‡ from the beginning, since the experiment is a dynamical one before it becomes a thermometrical one, and the dynamical results have all along been taken for thermometrical ones. The values of the ratio ( $k$ ) of the two specific heats of gases so determined are consequently nugatory, and this part of the discussion in Dr. Le Conte's paper falls to the ground.

With respect to my own method of investigating the fundamental differential equations for the motions of fluids in the Philosophical Magazine for March 1851, "On the Consideration of their Atomic Constitution," I believe it is sufficient to insist that the same atomic theory which is the basis of modern chemistry must be considered in other cases of physical problems which are affected by it; and hydrodynamical problems unquestionably are so affected.

With respect to my theoretical value for the velocity of sound from atomic considerations, which is 1122·2 feet per second, or 32·2 feet per second more than 1090 feet, the received velocity at the freezing temperature, I find reasons for believing it to be the correct result when sound passes over still water, from the experience of the artillery officers, and is therefore the normal velocity. The distance of an object which is to be fired at, is often found by noticing the interval of time between seeing the flash and hearing the report of a gun at the object; and it is found that the gun, the shot of which is required to strike the object, must be elevated rather more when the sound has tra-

\* Phil. Mag. for January 1862.

† Ibid. September 1853.

‡ Ibid. October 1862.



velled over water than when it has travelled over land,—showing that the object was more distant than would be given by a calculation with the velocity of sound ordinarily used. This is explained by the velocity being slightly greater over still water than over the rough surface of the land; and we know that sounds are heard much further over still water than over land. The artillery solution of the fact, that “*the water attracts the shot*,” we may leave to their own system of natural philosophy.

The challenge which I gave in my paper “On Hydrodynamics” in the Philosophical Magazine for March 1851, that the properties of diverging streams of air in Roberts’s experiment (often called Clement’s experiment), described in the fifth volume of the new series of the Transactions of the Literary and Philosophical Society of Manchester, could not be solved by the received equations for fluid motion, has never been answered; nor has any valid solution, except my own based on atomic considerations, been published that I am aware of. If any method of treating hydrodynamics fails to solve so simple a problem, it has no claims to our respect.

As to the mutual support which the theory of sound and the mechanical theory of heat afford to each other, as stated by Dr. Le Conte, the less that is said about it the better, whilst the objection\*, “His method of calculating the specific heat of gases, from the mechanical effects which they produce, leads to results which materially differ from those obtained by the observations of De la Roche and Bérard,” and, we may add, now from those obtained by M. Regnault also, is unanswered.

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## XXI. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from vol. xxvi. p. 542.]

**Z**WENGER and Bodenbender have contributed the results of an investigation on cumarine†. It had already been observed that the accounts given of the properties of this substance differ with its source, and so considerably that the bodies cannot be identical. They investigated first of all the cumarine from *Melilotus*. The aqueous extract of this substance is treated with ether until the solution has scarcely an acid reaction, and then the ether is removed by distillation. A greenish, partly slimy and partly crystalline mass is left, which dissolves by repeated treatment with boiling water, with the exception of a small residue. On

\* Annual Report of the Progress of Chemistry and the Allied Sciences, by Professors Leibig and Kopp. The translation by Drs. Hofmann and Benze Jones. Vol. iii., for 1849, p. 20.

† Liebig’s *Annalen*, June 1863.

cooling, provided the solution be dilute, large well-defined crystals are deposited, while in the mother-liquor there is a new crystallizable acid which the authors name *melilotic acid*.

The crystals above mentioned are readily purified by one or two crystallizations from alcohol, and consist then of transparent colourless rhomboidal plates, or, when rapidly formed, of silky needles. The body has the odour of cumarine from tonka beans, but somewhat feebler. The solution has an acid reaction, and produces precipitates with basic acetate of lead and with sesquichloride of iron, in which respect it differs from the cumarine from tonka beans. The composition was found on analysis to be  $C^{36}H^{16}O^{10}$ . On further investigation, this substance was found to be a compound of cumarine with melilotic acid; for on treating it with ammonia, the acid was dissolved out and the residue, purified by recrystallization, was found in all its properties, melting-point, neutral reaction, and crystalline form, to be identical with ordinary cumarine.

To obtain melilotic acid, the aqueous solution of the above-mentioned ethereal extract is treated with acetate of lead, by which free melilotic acid, as well as that united with cumarine, are precipitated; the precipitate is extracted with ether and alcohol to remove any free cumarine; the residue, being diffused in warm water, is treated with sulphuretted hydrogen, and the filtrate evaporated on the water-bath until oily drops begin to form. On cooling, the residue cools to a brownish coloured mass of crystals, which are then purified.

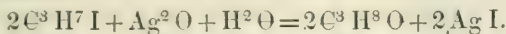
Melilotic acid affects the form of small prisms, which are soluble in water, alcohol, and ether, and its solution has a distinctly acid reaction. It decomposes, on being heated, into water and an oil, which is the anhydride. By nitric acid it is principally changed into picric acid. Its salts are mostly crystallizable, and soluble in alcohol and ether.

Its formula is  $C^{18}H^{10}O^6$ , and it unites with cumarine,  $C^{18}H^6O^4$ , to form melilotate of cumarine,  $C^{36}H^{16}O^{10}$ , without the elimination of water. It is probably bibasic, as seems from its decomposition into anhydride and water by being heated, and also by its forming acid salts. It is constituted according to the general formula  $C^nH^{n-8}O^6$ , and accordingly belongs to the salicylic acid series. It is isomeric with phloretic acid, and only differs from cumaric acid,  $C^{18}H^8O^6$ , by containing 2H more.

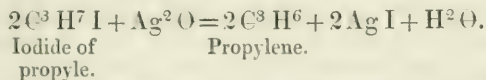
Erlenmeyer found\* that iodide of propyle was formed by the action of excess of hydriodic acid on glycerine. This iodide, when treated with oxalate of silver, yields oxalate of propyle, from which propylic alcohol,  $C^3H^8O$ , was obtained by the action

\* *Zeitschrift für Chemie*, part 13, 1863.

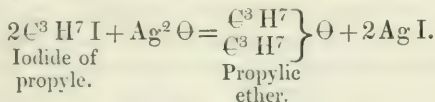
of ammonia. Erlenmeyer expected by the action of the iodide on oxide of silver to obtain propylic alcohol, in accordance with the equation



He accordingly heated iodide of propyle with moist oxide of silver in a flask connected with a condensing arrangement. He observed the disengagement of a gas which a subsequent experiment showed to be propylene, the formation of which may be thus expressed:—

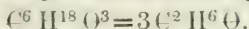


The contents of the flask consisted of iodide of silver with two liquids. After adding water, the flask was heated until the thermometer rose to 100°. The distillate consisted of two separate layers, an aqueous inferior layer, on which swam a mobile body with a peppermint odour. Water was added to the two liquids, whereby a larger quantity of this lighter liquid was separated. Washed with water, dried over chloride of calcium, and rectified, it passed over completely between 60° and 62°, and was found on analysis to consist of propylic ether,  $\text{C}^6\text{H}^{14}\Theta$ . The formation may be expressed as follows:—



Its formation may also be otherwise expressed; but in any case its production in a liquid containing excess of water is of interest to those who consider that ethers only result from a dehydrating process.

The aqueous layer formed in the above operation was distilled on the water-bath, the distillate saturated with carbonate of potash, and the liquid separated from the carbonate of potash dehydrated by means of anhydrous sulphate of copper. When distilled it passed over at 80° C., and was found on analysis to have the composition of ordinary alcohol,  $\text{C}^2\text{H}^6\text{O}$ . The formation of this alcohol appeared, however, so unusual, that Erlenmeyer was led to the idea that the substance might be a special hydrate of propylic alcohol, of the composition  $2\text{C}^3\text{H}^8\Theta + \text{H}^2\text{O}$ , which represents threefold the composition of ordinary alcohol; for



It was accordingly again dehydrated over dry sulphate of copper and, on rectification, distilled at 82°. The analysis gave, however, too small an amount of carbon for propylic alcohol. Erlen-



meyer accordingly heated it with iodide of phosphorus, from which he obtained iodide of propyle, agreeing in its properties with ordinary iodide. To free the alcohol from water, it was treated with sodium; and its boiling-point after this operation was found to be  $85^{\circ}$ . Berthelot states that the propylic alcohol obtained from propylene boils at  $81^{\circ}$  to  $82^{\circ}$ .

By the action of biniodide of phosphorus,  $\text{PI}_2$ , on glycerine, iodide of allyle,  $\text{C}^6\text{H}^5\text{I}$ , is obtained. Dragendorff recommends\* a method for the preparation of this substance, which consists in the simultaneous action of amorphous phosphorus and iodine on concentrated glycerine. Woieikoff†, who has investigated this method, using various proportions, has found that the body obtained is not iodide of allyle,  $\text{C}^6\text{H}^5\text{I}$ , but iodide of propyle,  $\text{C}^6\text{H}^7\text{I}$ . This difference in the results might have arisen from the glycerine used having contained more water; but he found that the same glycerine, when treated with iodide of phosphorus, yielded iodide of allyle with propylene, and not a trace of iodide of propyle. Hence the action of a mixture of iodine and amorphous phosphorus, in the proportions in which they form biniodide of phosphorus, on glycerine is different to that which iodide of phosphorus already formed exercises on the same substance.

Schmitt‡ has found that when cinnamic acid is placed in bromine vapour at ordinary temperature, it absorbs an amount of bromine corresponding to 2 equivs. of bromine for 1 equiv. of cinnamic acid. The body thus formed, *bibromocinnamic acid*,  $\text{C}^{18}\text{H}^8\text{Br}^2\text{O}^4$ , dissolves readily in warm alcohol, and crystallizes out in rhombic plates. When heated in a closed vessel, it decomposes with evolution of hydrobromic acid vapours, while an agreeable aromatic odour resembling hyacinths is perceived. The acid is dissolved, but not otherwise changed, by water at ordinary temperature; but when heated, the solution becomes turbid, and an aromatic odour resembling hyacinths is perceived; on distillation, an oil passes over which possesses this odour in the highest degree. The nature of this oil, which appears to be formed in all decompositions of brominated cinnamic acid, has not been ascertained; but it appears to be a brominated hydrocarbon.

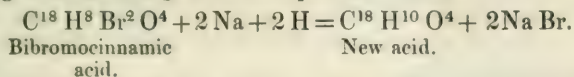
With baryta, bibromocinnamic acid forms a salt which under the microscope is seen to consist of minute crystals. It has the composition  $\text{C}^{18}\text{H}^7\text{BaBr}^2\text{O}^4$ . A soda-salt was also prepared of analogous composition.

\* *Pharmaceutische Zeitschrift für Russland*, No. 2, 1862.

† *Zeitschrift für Chemie*, part 3, 1863.

‡ *Liebig's Annalen*, May and September 1863.

The bromine in the acid may be replaced by hydrogen, by adding sodium-amalgam to its aqueous solution.



To the product of the action, dilute sulphuric acid is added until a distinct acid reaction is set up; a compact mass of crystalline structure separates out after continued standing, which, when recrystallized, forms large lustrous needles. This new acid has an intensely acid taste, is insoluble in water, but readily so in alcohol and ether. At a gentle heat it melts readily to a colourless liquid, which forms, on cooling, a crystalline mass. From the formula of this new acid,  $\text{C}^{18} \text{H}^{10} \text{O}^4$ , it is either the missing link between toluic and cuminic acids in the benzoic acid series, or it is isomeric with this acid. Schmitt assigns to it the name *cymolic acid*, as the corresponding hydrocarbon bears the name *cymole*.

Anteriorly to the publication of Schmitt's paper, Erlenmeyer, led by theoretical considerations as to its constitution, had investigated\* the action of nascent hydrogen on cinnamic acid. To an aqueous solution of cinnamic acid a large excess of sodium-amalgam was added; and after the action was over, hydrochloric acid in excess was added, whereby a substance was separated in clear drops which solidified on agitation. This was an acid, and was converted into the ether by passing hydrochloric acid gas through its alcoholic solution. The ether thus obtained was found to boil at  $243^\circ$ , and had the composition  $\text{C}^{11} \text{H}^{14} \text{O}^2$ ; from which it follows that the composition of the acid is  $\text{C}^9 \text{H}^{10} \text{O}^2$ , and is thus isomeric and probably identical with Schmitt's acid. Erlenmeyer assigns to it the name *homotoluic acid*; and is engaged on a further investigation of its derivatives.

Saytzeff has investigated the action of hydriodic acid on anisic acid†. When anisic acid is heated with hydriodic acid in a closed tube at  $125^\circ$  to  $130^\circ \text{C.}$  for twelve to sixteen hours, the decomposition is tolerably complete; a quantity of iodide of methyle is formed, which on opening the tube can be distilled off in the water-bath.

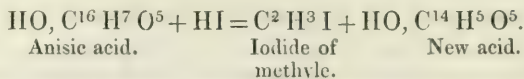
When the residue in the tube, after expelling the iodide of methyle, is heated in an open porcelain dish in order to get rid of excess of hydriodic acid, a strongly coloured acid crystallizes out, on cooling, which, by treatment with animal charcoal and recrystallization, can be obtained colourless.

The acid forms monoklinometric prisms, which are easily

\* Liebig's *Annalen*, vol. cxxi. p. 376.

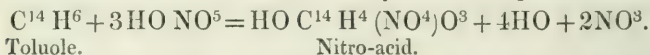
† Ibid. August 1863.

soluble in hot water, alcohol, and ether, at 100° lose water of crystallization, and at 210° melt with partial decomposition. The formula of the dehydrated acid is  $C^{14}H^8O^6$ , which is the same as that of oxybenzoic and salicylic acids. Its formation may be thus expressed:—



The new acid differs essentially from salicylic acid, more especially in that it does not give with perchloride of iron the violet colour characteristic of the latter acid. It also differs from oxybenzoic acid, although it stands nearer to it in many respects than to salicylic acid. Saytzeff names it *Paraoxybenzoic acid*. It is a strong acid, and forms with the oxides of zinc and cadmium and with the alkaline earths well-defined salts.

When crude nitrobenzole, prepared from benzole containing toluole, is treated with dilute soda lye, and the alkaline liquid supersaturated with hydrochloric acid, an acid precipitate is obtained, which, after being purified by recrystallization, has much similarity with benzoic acid. Fischer, who made this observation\*, had the opportunity of examining a specimen of this substance which had been obtained as a by-product in a manufactory. It was found on analysis to have the composition of nitrobenzoic acid,  $C^{14}H^5NO^8$ , and is obviously a product of oxidation of toluole; its formation may be thus expressed:—



This new acid, although identical in composition with nitrobenzoic acid, differs materially in a great many points, most remarkably in the melting-point, which in the case of nitrobenzoic acid is 227° C., and in the new acid (which he names *paranitrobenzoic acid*) is 240° C. The salts of the two acids differ also.

From their isomerism, it might be supposed that nitrobenzoic and paranitrobenzoic acids stood to each other in the same relations as benzoic and salicylic acids†. To decide this point, the simplest plan was to convert the paranitrobenzoic acid into the amido-acid, and this by treatment with nitrous acid into the corresponding oxyacid, which ought then to be identical with salicylic acid. Experiments have not confirmed this supposition, but they have led to the discovery of a new acid isomeric with amido-benzoic acid, and of an acid isomeric with oxybenzoic and sali-

\* Liebig's *Annalen*, August 1863.

† Phil. Mag. vol. xx. p. 383.



cylic acids. These acids he names *paramidobenzoic* and *paroxybenzoic* acids; and the interesting result was obtained, that the paroxybenzoic acid formed was identical with that obtained in another way by Saytzeff (*vide supra*).

The method by which the base piperidine has hitherto been prepared, by the decomposition of piperine by potash, is so unproductive as to make investigation of the former base very expensive. Wertheim has now\* found that when the alcoholic solution of pepper is directly treated with potash, the whole of the piperine which it contains is converted into piperidine; after distilling off the alcohol, adding water and again distilling, an alkaline distillate is obtained containing, besides a large quantity of ammonia, piperidine and a small quantity of etherial oil of pepper. When this distillate, neutralized with acid, is evaporated to dryness and exhausted with absolute alcohol, the piperidine salt is dissolved out along with the oil. This latter is removed by evaporating again, dissolving in the smallest quantity of water, and filtering through a moistened filter. The small quantity of oil which passes through is finally got rid of by repeatedly evaporating the solution to dryness in the water-bath with addition of water. On subsequently treating the salt with potash, the base is separated as an oily layer, which is rectified. 28 kilogrammes of pepper thus treated gave 350 grms. pure piperidine.

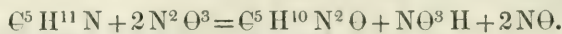
Nitrous acid acts energetically on piperidine; it is absorbed with such rapidity that the delivery-tube of the gas must not dip in the liquid, and during the whole operation the liquid must be carefully cooled. When the excess of nitrous acid is removed by passing carbonic acid through at a temperature of 30° to 40° C., a clear yellow oily liquid of an acid aromatic odour is left.

This liquid was purified by several methods, one of which was the following. It was dissolved in water, and potash gradually added, by which an oil was separated. This was washed with very dilute hydrochloric acid, dried over chloride of calcium and rectified. This substance was distilled below its boiling-point, at 160° to 180° C.; and as the distillate was still somewhat alkaline, the same process was repeated.

The substance thus purified is neutral, tolerably soluble in water, more so in dilute acids, and very much so in strong acids, from which it is precipitated by the addition of water. It rapidly evaporates at 160° to 180° C.; at 205° C. the body passes over almost to the last drop. At 240° it begins to boil, but at the same time gradually decomposes. Its composition is expressed by the formula  $C^5H^{10}N^2O$ , which was confirmed by a vapour-density determination.

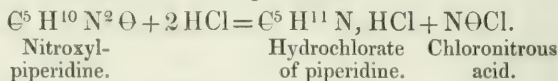
\* Liebig's *Annalen*, July 1863.

In its formation, piperidine simply assimilates nitrous acid and liberates water, which, acting upon some nitrous acid, produces nitric acid. The following equation expresses both phases of the action :—



Piperidine. Nitrous acid. New body. Nitric acid.

The body is thus piperidine in which hydrogen is replaced by binoxide of nitrogen, and it may be named *Nitroxylpiperidine*. By the action of nascent hydrogen, the binoxide can be removed and replaced by hydrogen, and the original body restored. The same change may be effected by hydrochloric acid. When nitroxylpiperidine is heated to  $100^{\circ}$  in a current of this gas, it is first changed into a crystalline mass, which by the further action of the acid is again liquefied; and on continuing the action, maintaining the temperature at  $100^{\circ}$  C., a yellow gas is disengaged, and a crystalline mass left. This was found to be hydrochlorate of piperidine, and a careful investigation of the gas showed that it was chloronitrous acid. The formation of these two substances is thus expressed:—



Nitroxyl-  
piperidine.

Hydrochlorate  
of piperidine.

Chloronitrous  
acid.

By the substitution of binoxide of nitrogen for hydrogen, the basic properties of the piperidine are greatly diminished, yet not entirely destroyed; for direct experiments of Wertheim showed that the substance probably absorbed a quantity of hydrochloric acid corresponding to one equivalent, forming a crystalline body.

Hlasiwetz and Gilm\* have described a hydrogenized derivative of berberine obtained by the action of nascent hydrogen upon that base. In a spacious flask connected with a condenser, a mixture is introduced of berberine, water, pure sulphuric acid, glacial acetic acid, a large quantity of zinc, and a few pieces of platinum, and the whole heated to boiling. The original dark golden colour of the solution becomes gradually lighter; and the reaction is interrupted when no further change takes place, which is generally after the lapse of one or two hours. The best method of obtaining the new product consists in filtering this liquid, and adding to the filtrate an excess of a saturated solution of common salt, by which the new body is entirely deposited as hydrochlorate in the form of a very difficultly soluble precipitate. This is washed and pressed, dissolved in boiling alcohol, and ammonia added. On cooling, the new base crystal-

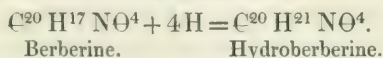
\* Liebig's *Annalen*, Supplement, vol. ii. No. 2.

lizes out; the crystals are washed with water and purified by recrystallization.

The new body can also be produced by the action of sodium-amalgam on berberine; but this method is not advantageous.

The new body is a base; it crystallizes in forms belonging to the monoklinoedric system. Originally colourless, its solution acquires by continuous boiling, or by standing in the air, an orange-yellow tint. It forms with acids a series of well-crystallizing salts, the analyses and detailed descriptions of which are given by the authors.

The composition of the base is  $\text{C}^{20} \text{H}^{21} \text{N}\Theta^4$ ; and as its formation from berberine is unattended by the production of any other essential product, its generation must be very simple. Hydrogen could only have acted as a reducing agent, or have been simply assimilated. To express the formation of the new substance, Hlasiwetz and Gilm have been compelled to assume for the formula of berberine  $\text{C}^{20} \text{H}^{17} \text{N}\Theta^4$ , which agrees with the published analyses, and which Perrins has also recently adopted\*. The formation of the new base, which the authors name *hydroberberine*, is then thus simply expressed:—



From hydroberberine, berberine may be regenerated by the process by which Fritzsche succeeded in transforming harmaline into harmine. Hydroberberine is dissolved in a warm mixture of hydrochloric acid and alcohol, and some nitric acid gradually added; the liquid acquires the colour of the berberine solution, and, on warming, red vapours are disengaged. On cooling, crystals are deposited, which are hydrochlorate of berberine.

## XXII. On the Hexatomicity of Ferricum and Aluminium.

By WILLIAM ODLING, M.B., F.R.S., Sec. Chem. Soc.†

UNTIL lately the chlorides of ferricum and aluminium were ordinarily represented as sesquichlorides by the formulæ  $\text{Fe}^2 \text{Cl}^3$  and  $\text{Al}^2 \text{Cl}^3$  respectively. Doubling the atomic weights of the constituent metals, as advocated by Messrs. Cannizzaro and Wurtz and now generally admitted to be necessary, these formulæ become of course  $\text{Fe} \text{Cl}^3$  and  $\text{Al} \text{Cl}^3$ . But there are undoubtedly very strong reasons for believing that both formulæ express only half molecules of the respective compounds, and that the entire molecules ought to be represented by the formulæ  $\text{Fe}^2 \text{Cl}^6$  or  $\text{Fe}^{\text{vi}} \text{Cl}^6$ , and  $\text{Al}^2 \text{Cl}^6$  or  $\text{Al}^{\text{vi}} \text{Cl}^6$ , in which the

\* Journal of the Chemical Society, vol. xv. p. 339.

† Communicated by the Author.

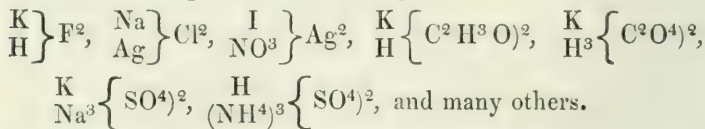


atoms of ferricum and aluminium are considered as hexatomic. It seems to me, however, that, in the present state of knowledge, the arguments for and against this hexatomicity are pretty evenly balanced, or at any rate that those in its favour do not preponderate so decidedly as to warrant us in hastily giving up our old established notions.

As I understand them, the chief arguments for the hexatomicity of ferricum and aluminium are deducible from the vapour-densities of the two chlorides ( $\alpha$ ), from the existence of certain basic and complex salts of ferricum ( $\beta$ ), and from the properties of aluminium-ethyl, &c. ( $\gamma$ ).

$\alpha$ . If we regard the chlorides of ferricum and aluminium as trichlorides, their respective molecules will have only half the volume of the molecule of hydrogen,  $H^2$ , chlorhydric acid,  $H\ Cl$ , and 99 per cent. of all volatile bodies; whereas if we regard them as hexachlorides, the gaseous volumes of their molecules will be perfectly normal. On the other hand, it may be argued that the molecules of arsenious anhydride,  $As^2\ O^3$ , arsenicum,  $As^2$ , and phosphorus,  $P^2$ , are deliberately represented by formulæ corresponding to volumes only half as great as those of the molecules of hydrogen and chlorhydric acid, and that possibly the same cause or want of knowledge which renders the vapour-densities of these last bodies anomalous may render those of the trichlorides of ferricum and aluminium similarly anomalous also.

$\beta$ . Several complex and basic salts of ferricum have been described particularly by M. Scheurer-Kestner, whose respective constitutions are in harmony with the formula  $Fe^2\ Cl^6$  or  $Ffe\ Cl^6$ ; and certainly cannot be brought into harmony with the formula  $Fe\ Cl^3$ . On the other hand, it may be urged that some, and apparently the best-defined, of M. Scheurer-Kestner's new bodies are in harmony with the formula  $Fe\ Cl^3$ , and that several very definite compounds are known (related to fluorhydric acid, to chloride of sodium, to nitrate of silver, and to the acetic, oxalic, and sulphuric acids for instance) whose respective constitutions are not in harmony with the recognized formulæ of the several types, but only with the doubles of those formulæ—such compounds, for example, as the following:



$\gamma$ . What little is known of aluminium-ethyl and aluminium-methyl seems certainly to be in favour of the formula  $Al^3\ Cl^6$ , or  $Al\ Cl^6$ . But, on the other hand, these bodies have been too imperfectly examined to warrant the deduction of any weighty argu-

ment from considerations based on their composition and behaviour. Cahours represents an ethyl-iodide of aluminium by the formula  $\text{Al}^2 \text{Et}^3 \text{I}^3$ , just as Bunsen represents an iodide of nitrogen by the formula  $\text{N}^2 \text{H}^3 \text{I}^3$ . A more complete examination, however, of aluminium-ethyl and -methyl would doubtless go far to settle the question of hexatomicity regarded merely as a question of fact.

What I conceive to be the arguments opposed to the hexatomicity of aluminium and ferricum are drawn from the specific heat of aluminium ( $\alpha$ ), from the non-existence of any inferior chloride of aluminium ( $\beta$ ), from the improbability of an increment of chlorine combining two molecules of ferrous chloride into one molecule of ferric chloride ( $\gamma$ ), and from the series of iron chlorides requiring a trichloride for its completion ( $\delta$ ).

$\alpha$ . The proportion of aluminium which exists in the molecule of aluminic chloride is the smallest proportion of aluminium that is known to exist in any combination whatever, and accordingly ought to be regarded as an atomic proportion. But if this proportion be taken at 55 parts, the atom of aluminium will have a specific heat twice as great as that of the atom of any other element. Thus the atomic heats of certainly all the metals will be expressed by a number approaching more or less nearly to 6.2, except indeed the atomic heat of aluminium, which will be expressed by the number 11.7. Now there does not appear to be any peculiarity in the general behaviour of aluminium which would warrant us in according to it such an anomalous atomic heat. If, however, the 55 parts of aluminium contained in the hexatomic molecule of aluminic chloride be regarded as two atoms, then aluminium will present the yet more striking and inexplicable peculiarity of never entering into combination save in the proportion of two inseparable atoms.

$\beta$ . There are several elements of which the lowest known chloride is a trichloride, but none of which it is a hexachloride. The hexachlorides of iridium,  $\text{Ir Cl}^6$ , of osmium,  $\text{Os Cl}^6$ , and of dicarbon,  $\text{C}^2 \text{Cl}^6$ , for instance, proceed each from a tetrachloride and dichloride, and so constitute each the third term of a series of chlorides; whereas hexachloride of aluminium,  $\text{Al}^2 \text{Cl}^6$ , or  $\text{Al Cl}^6$ , would be the first or lowest chloride of aluminium. Here again there is no peculiarity in the general behaviour of aluminium, and certainly no extreme electro-negative tendency comparable to that of chromium and molybdenum, whose inferior chlorides were for a long time concealed, which would suggest or accord with its possession of a hexatomic lowest chloride.

$\gamma$ . It is scarcely conceivable, and certainly contrary to all analogy, that continuous chlorination should lead to the synthetic formation of a complex molecule. For the action of chlo-

rine and oxygenants in general is always to effect the breaking up, never, so far as I know, the building up of compounds, save of intermediate or diameric compounds such as green hydrokinone. That the first action of chlorine upon iron should be to produce a dichloride,  $\text{Fe Cl}^2$ , and that its continued action should have the effect of uniting two molecules of this chloride into one molecule of a non-diameric higher chloride according to the equation,  $2 \text{Fe Cl}^2 + \text{Cl}^2 = \text{Fe}^2 \text{Cl}^6$  or  $\text{Ffe Cl}^6$ , seems most improbable; as does also the oxidation of two molecules of yellow prussiate of potash,  $2 \text{K}^4 \text{Fe}^{\text{II}} \text{Cy}^6$ , into one molecule of red prussiate of potash,  $\text{K}^6 \text{Ffe}^{\text{VI}} \text{Cy}^{12}$ , instead of  $2 \text{K}^3 \text{Fe}^{\text{III}} \text{Cy}^6$ .

δ. It appears that the atoms of one class of elements unite respectively with 1, 3, 5, and 7 atoms of chlorine. These elements I call perissads. They yield oxides having the general formulæ  $\text{M}^2\text{O}$ ,  $\text{M}^2\text{O}^3$ ,  $\text{M}^2\text{O}^5$ , and  $\text{M}^2\text{O}^7$ . The atoms of another class of elements unite with 2, 4, and 6 atoms of chlorine respectively. These elements I call artiads. They form oxides having the general formulæ  $\text{M}^2\text{O}^2$ ,  $\text{M}^2\text{O}^4$ , and  $\text{M}^2\text{O}^6$ , or perhaps  $\text{M O}$ ,  $\text{M O}^2$ , and  $\text{M O}^3$ . But some few metals which I call periss-artiads, copper and manganese, for instance, both belonging to the same group as iron, which indeed constitutes their intermediate term, are indisputably both perissad and artiad. Thus we have both cuprous chloride,  $\text{Cu Cl}$ , and cupric chloride,  $\text{Cu Cl}^2$ , and both manganic and permanganic acids corresponding respectively to a hexachloride of manganese,  $\text{Mn Cl}^6$ , and a heptachloride of manganese,  $\text{Mn Cl}^7$ . The analogy of cuprous to silver compounds, and of permanganates to perchlorates, seems to me conclusive of the perissad character of copper and manganese in some of their combinations. Now taking copper, iron, and manganese, with their congeners, as forming between them a complete series of chlorides, we should have produced by the action of chlorine upon the typical metal, first  $\text{M Cl}$ , represented by cuprous chloride, sub-nickelous chloride, and sub-ferrous iodide; then  $\text{M Cl}^2$ , represented by the cupric, ferrous, and manganeous chlorides; then  $\text{M Cl}^3$ , represented by the so-called sesquichlorides of copper (?), iron, and manganese; then  $\text{M Cl}^4$ , represented by tetrachloride of manganese (?), pyrolusite, and iron pyrites; then  $\text{M Cl}^5$ , corresponding to a well-defined pentoxide of chromium; then  $\text{M Cl}^6$ , represented by the ferric and manganic acids, and by oxychloride of chromium; and lastly  $\text{M Cl}^7$ , represented by heptachloride of manganese and permanganic acid, thus:—

Perissads.	Artiads.
$\text{M Cl}$	$\text{M Cl}^2$
$\text{M Cl}^3$	$\text{M Cl}^4$
$\text{M Cl}^5$	$\text{M Cl}^6$
$\text{M Cl}^7$	



The formulæ of the extreme perissad compounds  $MCl$  and  $MCl^7$  being indisputable, that of the intermediate perissad compound  $MCl^3$  would seem to be at any rate not improbable.

I will venture to add yet one more argument on this side of the question, based, I must confess, rather upon a personal impression than a demonstrable fact. I am inclined to think that alumina is an analogue of boric anhydride; that boracite and spinelle belong to the same type; and that in several complex aluminous silicates a portion of the alumina is replaced isomorphously by boric anhydride, or rather that a portion of the aluminium is replaced by boron. If this view is correct, the molecule of aluminic chloride ought probably to be represented by the formula  $AlCl^3$ , similar to the well-established formula for the molecule of boric chloride,  $BCl^3$ , despite the want of relation between the observed vapour-densities of the two compounds. I say "ought probably," because isomorphism does sometimes obtain between elements of different atomicities, as instanced by silver blende,  $Ag^{12}S$ , and galena,  $Pb^{11}S$ , for example; whence it is quite possible that  $B^{11/2}O^3$  may be isomorphous with  $Al^{vi}O^3$ . In reference to this point, it is perhaps worthy of notice that the difference between the atomic weights of boron (11) and aluminium (27.5) is substantially the same as that between the atomic weights of fluorine and chlorine, oxygen and sulphur, nitrogen and phosphorus, carbon and silicon, glucinum and magnesium, lithium and sodium, and sodium and potassium respectively.

I have put together these few remarks, not at all with the view of deciding the matter, but merely of calling attention to the difficulties by which it is beset. Indeed my own mind is far from made up on the subject; and if I continue to use the time-honoured triatomic formulæ, it is not from conceiving them to have a proved superiority, but because their inferiority has not yet been established beyond all question, as well as because they are simpler and more familiar to me.

### XXIII. Remarks on Chemical Notation.

By WILLIAM ODLING, M.B., F.R.S., Sec. Chem. Soc.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

IN addition to a perfectly gratuitous meddling with me, and an uncalled for sneer at the feeble intelligence of chemists, Mr. Waterston's original letter to you upon the subject of chemical notation contained an authoritative enunciation of certain supposed new views a long while since adopted, and of other undoubtedly old views a long while since abandoned by modern

chemists—those concerning the ratio of the molecular weights of water and caustic potash for example.

He affected to teach us that the molecules of the chemical elements are divisible in the act of combination, and that all gaseous molecules, elementary or compound, occupy the same bulk; the former doctrine satisfactorily established by the researches of more than a dozen years ago, and the latter, after a much chequered career, recognized for some time past as a very corner stone of chemical science, notwithstanding that in a Royal Institution lecture to a very general audience I endeavoured to explain it incidentally by what Mr. Waterston complimentary terms “ridiculously clear” illustrations.

Admitting that all gaseous molecules have the same bulk, the majority of chemists do not think it a matter of much consequence how that bulk should be expressed—whether by the apposition of two semicircles so as to form a single volume  $\Theta$ , as approved by Mr. Waterston, or by the apposition of two entire circles so as to form a double volume  $OO$ , as condemned by him, or in any other way. For myself, indeed, I am of opinion, with Laurent and several others, that a single-volume standard is in the abstract preferable, but, not regarding the point as one of any great importance, habitually employ a double-volume standard as affording certain practical advantages which those engaged in chemical practice can probably best appreciate. If Mr. Waterston, however, instead of declaiming against us from his rostrum will show us with civility and, if possible, on a footing of equality, that the point is of more importance than we at present deem it, he will assuredly find us well disposed to give his remarks an attentive consideration; but the question not being one with which I am in any special way identified, I must decline to enter into any personal discussion upon it, more particularly with such an antagonist as Mr. Waterston has now proved himself to be; for how can one discuss chemical questions with a gentleman whose knowledge of the literature and thought of modern chemistry has allowed him to regard the expression “hydrate of sodium” as being either a typographical error or an affected singularity”\*?

In reply to the query whether “it is possible that the first and most obvious consequence of the equality in bulk of gaseous molecules (namely that the ratios of vapour-densities are also the ratios of molecular weights) is a scaled book in Mr. Odling’s

\* Such expressions as hydrate of sodium, nitrate of potassium, sulphate of calcium, &c. are no longer confined to original memoirs or purely chemical treatises, but are used throughout in such popular works as the Jury Report of the International Exhibition of 1862, Class II. Section A. Chemical Products and Processes, &c.

school," I answer that it is (as Mr. Waterston perfectly well knows) quite impossible that such a consequence should be concealed from any one, even if it had not been brought into prominent notice by Ampère nearly half a century ago. But I am still unconvinced that the numerical expression for the molecular weight of any particular body is an abstract unchangeable expression, and not a relative one varying with the standard of comparison adopted,—that the molecular weight of water, for instance, will not be 9, or 18, or 112·5, accordingly as the molecular weight of hydrogen is taken as 1 (Dalton), or 2 (Laurent and Gerhardt), or 12·5 (Berzelius). Mr. Waterston inconsiderately reproached me with having represented the molecular weight of water by the number 18 in opposition to its vapour-density, whereas my argument was that if the molecular weight of hydrochloric acid is 36·5 (and by consequence the molecular weight of hydrogen 2), then upon chemical grounds the molecular weight of water will be 18 in accordance with its vapour-density, and not 9 in opposition thereto. Dalton and Berzelius alike represented the ratio of the molecular weights of water and hydrochloric acid as being  $\frac{9}{36\cdot5}$ , the ratio of their vapour-densities being  $\frac{13}{36\cdot5}$ . Gerhardt first recognized the admittedly true ratio of the molecular weights of water, hydrochloric acid, and hydrogen, and expressed it by the numbers 18, 36·5, and 2, which Mr. Waterston in his grand scheme of reform proposes to alter into 9, 18·25, and 1. He has absolutely nothing whatever to tell us about the molecular correlation of the bodies represented, but simply wishes to halve their accepted volumes, weights, and formulæ; which halving was considered long ago and not accepted, because any abstract advantages that it might possibly offer were not thought of sufficient consequence to counterbalance the practical inconveniences which would accrue from its adoption.

Although the determination of the ratio of the molecular weights of water and hydrate of potassium (or, in deference to Mr. Waterston, hydrate of potash) is considered by chemists to be a point of fundamental importance, and although the majority of them have come somewhat unwillingly to the conclusion that one molecule of water will form two molecules of the hydrate, yet Mr. Waterston, contemptuous of all that has been done or thought on the subject, boldly proclaimed in his first letter, "Now the fact is that two molecules of water are engaged in forming one molecule of the hydrate;" and the fact being thus declared, all discussion upon the point was of course at an end. But in his second letter he substitutes for his assertion of the alleged fact an argument which he considers to establish the



fact, and which is as follows:—He begins by saying that the proper formula for the molecule of water is  $\text{HO}^{\frac{1}{2}}$ , and that Gerhardt's formula  $\text{HH}\Theta$  can only be used at all by letting each H stand for a  $\frac{1}{2}$  part or a half-volume of hydrogen, and  $\Theta$  stand for 8 parts or a half-volume of oxygen; so that in his temporary use of the formula  $\text{HHO}$  (1 vol.), for the purpose of showing a *reductio ad absurdum*, he halves the usually received values of all the constituent symbols, which halving I am quite ready temporarily to accept. But in reference to Gerhardt's formula for potash,  $\text{KH}\Theta$ , he halves the usual values of the H and  $\Theta$ , making them stand for  $\frac{1}{2}$  a part and 8 parts of hydrogen and oxygen respectively; but does not halve the usual value of the K, which he still allows to stand for 39 parts of potassium; and having by this means doubled its actual proportion, exclaims triumphantly that the formula  $\text{HK}\Theta^*$  cannot be correct, because the 39 parts of constituent potassium would require all the 8 parts of constituent oxygen, whereby no oxygen would be left to combine with the  $\frac{1}{2}$  part of hydrogen,—thus intimating, with a recklessness which I forbear to characterize, that the formula for caustic potash deliberately adopted by a large and important section of British and foreign chemists is incompatible with the centesimal composition of the body represented—as is also the formula for anhydrous potash,  $\text{KK}\Theta$ , which is soon afterwards satisfactorily demolished by a similar hocus-pocus,—as if any conjuring with formulæ or arguments from notation could decide what is admitted to be a question of fact, or at any rate of interpretation of phenomena.

It seems that Mr. Waterston, taking his notions of Gerhardt's symbols at second hand, has confounded them with those of Berzelius, with whom O stood for 8 parts (100) of oxygen,  $\text{H}^2$  or H for 1 part (12·5) of hydrogen, and K for 39 parts (487·5) of potassium, with which values the composition of caustic potash could not possibly be represented by the formula  $\text{KHO}$ . But according to Gerhardt,  $\Theta$  stands for 16 parts of oxygen, H for 1 part of hydrogen, and K for 39 parts of potassium; or halving these values to please Mr. Waterston,  $\Theta$  will stand for 8 parts of oxygen, H for  $\frac{1}{2}$  a part of hydrogen, and K for 19·5 parts of potassium, which ratio is of course in harmony with the composition of the hydrate. Or adopting Mr. Waterston's fractional formulæ,  $\text{HO}^{\frac{1}{2}}$  (1 vol.) will represent the molecule of water,  $\text{H}^{\frac{1}{2}}\text{Br}^{\frac{1}{2}}\text{O}^{\frac{1}{2}}$  (1 vol.) the molecule of brominated water or

\* The old formula for caustic potash is  $\text{KO.HO}$ , or  $\text{KHO}^2$ , in which O stands for 8 parts of oxygen. Gerhardt's formula is  $\text{KH}\Theta$ , in which  $\Theta$  stands for 16 parts of oxygen, so that one  $\Theta$  having the value 16 is simply substituted for two Os having each the value 8.

hypobromous acid, whence  $H^{\frac{1}{2}}K^{\frac{1}{2}}O^{\frac{1}{2}}$  (hypothetical 1 vol.) should represent the molecule of potassic water or caustic potash,— $\frac{5.0}{2}$  parts of bromine, and  $\frac{3.9}{2}$  parts of potassium being the chlorous and basylous equivalents of  $\frac{1}{2}$  part of hydrogen, and having moreover the same specific heat as one another. Whether or not Gerhardt's view of the molecular ratio of water to caustic potash is correct, I trust I have not failed in making it "ridiculously clear" that Mr. Waterston's proof of its incorrectness is a transparent fallacy.

With the exception of a few strange vagaries, inconsistent with the unity of his own scheme, such as halving the accepted combining weight of hydrogen, and not halving that of potassium, halving the accepted formula of oxalic ether,  $C^2(C^4H^{10})O^4$ , and not halving that of formic ether,  $C(C^2H^5)HO^2$ , &c., I have nothing whatever to say against Mr. Waterston's V.-D. system, because it is neither more nor less than the system of Gerhardt as developed by his followers\*. But in Gerhardt's integral notation the smallest proportion of an isolated body is taken as two volumes, and the smallest proportion of a constituent element as one volume; whereas in Mr. Waterston's suggested fractional notation the smallest proportion of an isolated body is taken as one volume, and the usual smallest proportion of a constituent element as half a volume. This mere notational change may be perhaps of the greatest possible importance; but at present we have only been told and not shown so.

I am quite at a loss to understand why Mr. Waterston, in re-introducing his long-cherished views to public attention, should not have placed them at once on an independent basis, instead of first going out of his way to make an unfair attack upon me, and then using his attack as a peg on which to hang them. Should he at any time see fit to return to this subject—should he really think that the notions which he conceived some fifteen years ago, and which others had conceived many years before then, are unknown at the present day because in some parti-

\* In ninety-nine cases out of a hundred the vapour-density of a body, elementary or compound, harmonizes thoroughly with its chemical relations. In a few cases one and the same body, elementary or compound, has two distinct vapour-densities, only one of which, and that the least readily observable, harmonizes with its chemical relations; while in another few cases the body, elementary or compound, has a single observed vapour-density which does not harmonize with its chemical relations, and is only believed to have a second hitherto unobserved vapour-density which is in harmony therewith. Thus, in addition to its observed vapour-density of 62, phosphorus is believed to have an hitherto unobserved vapour-density of 31,—31 parts of phosphorus being the chemical representative of 14 parts of nitrogen, the observed vapour-density of which is 14; and so in a few other instances.

culars they have not been adopted, while in others they have been outgrown—I trust he will either discuss them from a purely abstract point of view, or at any rate select some other and more willing antagonist with whom to fight his battle.

I am, Gentlemen,

Your obedient Servant,

WILLIAM ODLING.

XXIV. *On Röber's Construction of the Heptagon.* By Sir WILLIAM ROWAN HAMILTON, LL.D., M.R.I.A., F.R.A.S., &c., Andrews Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland\*.

1. **I**N a recent Number of the Philosophical Magazine, observations were made on some approximate constructions of the regular heptagon, which have recalled my attention to a very remarkable construction of that kind, invented by a deceased professor of architecture at Dresden, Friedrich Gottlob Röber†, who came to conceive, however, that it had been known to the Egyptians, and employed by them in the building of the temple at Edfu. Röber, indeed, was of opinion that the connected triangle, in which each angle at the base is triple of the angle at the vertex, bears very important relations to the plan of the human skeleton, and to other parts of nature. But without pretending to follow him in such speculations, attractive as they may be to many readers, I may be permitted to examine here the accuracy of the proposed geometrical construction, of such an isosceles triangle, or of the heptagon which depends upon it. The closeness of the approximation, although short of mathematical rigour, will be found to be very surprising.

2. Röber's diagram is not very complex, and may even be considered to be elegant; but the essential parts of the construction are sufficiently expressed by the following formulæ: in which  $p$  denotes a side of a regular pentagon;  $r$ ,  $r'$  the radii of its inscribed and circumscribed circles;  $r''$  the radius of a third circle,

\* Communicated by the Author.

† The construction appears to have been first given in pages 15, 16 of a quarto work by his son, Friedrich Röber, published at Dresden in 1854, and entitled *Beiträge zur Erforschung der geometrischen Grundformen in den alten Tempeln Aegyptens, und deren Beziehung zur alten Naturerkenntniss*. It is repeated in page 20 of a posthumous work, or collection of papers, edited by the younger Röber, and published at Leipzig in 1861, entitled *Elementar-Beiträge zur Bestimmung des Naturgesetzes der Gestaltung und des Widerstandes, und Anwendung dieser Beiträge auf Natur und alte Kunstgestaltung*, von Friedrich Gottlob Röber, ehemaligen Königlich-Sächsischen Professor der Baukunst und Land-Baumeister. Both works, and a third upon the pyramids, to which I cannot at present refer, are replete with the most curious speculations, into which however I have above declined to enter.



concentric with but exterior to both;  $p'$  a segment of the side  $p$ ; and  $q, s, t, u, v$  five other derived lines. The result is, that in the right-angled triangle of which the inner diameter  $2r$  is the hypotenuse, and  $u, v$  supplementary chords, the former chord ( $u$ ) is *very nearly* equal to a side of a regular heptagon, inscribed in the interior circle; while the latter chord ( $v$ ) makes with the diameter ( $2r$ ) an angle  $\phi$ , which is *very nearly* equal to the vertical angle of an isosceles triangle, whercof each angle at the base is triple of the angle at the vertex. In symbols, if we write

$$u = 2r \sin \phi, \quad v = 2r \cos \phi,$$

then  $\phi$  is found to be very nearly  $= \frac{\pi}{7}$ . It will be seen that the

equations can all be easily constructed by right lines and circles alone, having in fact been formed as the expression of such a construction; and that the numerical ratios of the lines, including the numerical values of the sine and cosine of  $\phi$ , can all be arithmetically computed\*, with a few extractions of square roots.

(A) {	$(r + r')^2 = 5r^2$	$\frac{r'}{r} = 1.2360680$
	$p^2 = 4(r'^2 - r^2)$	$\frac{p}{r} = 1.4530851$
	$\frac{p'}{p} = \frac{r + \frac{1}{2}r'}{r + r'}$	$\frac{p'}{r} = 1.0514622$
	$q^2 = p^2 - p'^2$	$\frac{q}{r} = 1.0029374$
	$s^2 + ps = (p - q + r)^2$	$\frac{s}{r} = 0.8954292$
	$r''^2 = r^2 + s^2$	$\frac{r''}{r} = 1.3423090$
	$t^2 = \left(\frac{r'r''}{r}\right)^2 - (r'' - r)^2$	$\frac{t}{r} = 1.6234901$
	$u^2 = 2r(2r - t)$	$\frac{u}{r} = 0.8677672$
	$v^2 = 2rt$	$\frac{v}{r} = 1.8019379$
	$u = 2r \sin \phi$	$\sin \phi = 0.4338836$
$v = 2r \cos \phi$	$\cos \phi = 0.9009689$	

\* The computations have all been carried to several decimal places beyond what are here set down. Results of analogous calculations have been given by Röber, and are found in page 16 of the first-cited publication of his son, with the assumption  $p = \sqrt{3}$ , and with one place fewer of decimals.

3. On the other hand, the true septisection of the circle may be made to depend on the solution of the cubic equation,

$$8x^3 + 4x^2 - 4x - 1 = 0,$$

of which the roots are  $\cos \frac{2\pi}{7}$ ,  $\cos \frac{4\pi}{7}$ ,  $\cos \frac{6\pi}{7}$ . Calculating then, by known methods\*, to eight decimals, the positive root of this equation, and thence deducing to seven decimals, by square roots, the sine and cosine of  $\frac{\pi}{7}$ , we find, without tables, the values :

$$\cos \frac{2\pi}{7} = x = 0.62348980;$$

$$\sin \frac{\pi}{7} = \sqrt{\frac{1-x}{2}} = 0.4338837;$$

$$\cos \frac{\pi}{7} = \sqrt{\frac{1+x}{2}} = 0.9009689;$$

and these last agree so nearly with the values (A) of  $\sin \phi$  and  $\cos \phi$ , that at this stage a doubt may be felt, *in which direction does the construction err*. In fact, Röver appears to have believed that the construction above described was geometrically rigorous, and had been known and prized as such from a very remote antiquity, although preserved as a secret doctrine, entrusted only to the initiated, and recorded only in stone.

4. The following is an easier way, for a reader who may not like so much arithmetic, to satisfy himself of the extreme closeness of the approximation, by formulæ adapted to logarithms, but rigorously derived from the construction. It being evident that

$$r' = r \sec \frac{\pi}{5}, \quad \text{and} \quad p = 2r \tan \frac{\pi}{5},$$

\* Among these the best by far appears to be Horner's method,—for practically arranging the figures in the use of which method, a very compact and convenient form or scheme was obligingly communicated to me by Professor De Morgan, some time ago. We arrived independently at the following value, to 22 decimals, of the positive root of the cubic mentioned above :

$$\cos \frac{2\pi}{7} = 0.62348 \ 98018 \ 58733 \ 53052 \ 50.$$

I had however found, by trials, before using Horner's method, the following approximate value :

$$\cos \frac{2\pi}{7} = 0.62348 \ 98018 \ 587;$$

which was more than sufficiently exact for comparison with Röver's construction.

let  $\phi_1 \dots \phi_6$  be six auxiliary angles, such that

$$\begin{aligned} r' &= 2r \tan \phi_1, & p' &= p \sin 2\phi_2, & p - q &= r \tan^2 \phi_3, \\ p - q + r &= \frac{1}{2}p \tan 2\phi_4, & s &= r \tan 2\phi_5, & r(r'' - r) &= r'r'' \sin \phi_6; \end{aligned}$$

we shall then have the following system of equations, to which are annexed the angular values, deduced by interpolation from Taylor's seven-figure logarithms, only eleven openings of which are required, if the logarithms of two and four be remembered, as they cannot fail to be by every calculator.

$$(B) \left\{ \begin{array}{ll} \cot \phi_1 = 2 \cos \frac{\pi}{5} & \phi_1 = 31^\circ 43' 2.91 \\ \sin 2\phi_2 = \cos^2 \phi_1 & \phi_2 = 23^\circ 10' 35.52 \\ \tan^2 \phi_3 = 4 \sin^2 \phi_2 \tan \frac{\pi}{5} & \phi_3 = 33^\circ 51' 31.90 \\ \cot 2\phi_4 = \cos^2 \phi_3 \tan \frac{\pi}{5} & \phi_4 = 31^\circ 41' 39.37 \\ \tan 2\phi_5 = 2 \sin^2 \phi_4 \sec 2\phi_4 \tan \frac{\pi}{5} & \phi_5 = 20^\circ 55' 15.93 \\ \sin \phi_6 = \sin^2 \phi_5 \cot \phi_1 & \phi_6 = 11^\circ 54' 22.60 \\ \cos^2 \phi = \cos \phi_6 \sec 2\phi_5 \tan \phi_1 & \phi = 25^\circ 42' 51.4. \end{array} \right.$$

It is useless to attempt to estimate hundredths of seconds in this last value, because the difference for a second, in the last logarithmic cosine, amounts only to ten units in the seventh place of decimals, or to one in the sixth place. But if we thus confine ourselves to tenths of seconds, a simple division gives immediately that final value, under the form

$$\frac{\pi}{7} = \frac{180^\circ}{7} = 25^\circ 42' 51''.4;$$

it appears therefore to be difficult, if it be possible, to decide by Taylor's tables, whether the equations (B), deduced from Röber's construction, give a value of the angle  $\phi$ , which is *greater* or *less* than the *seventh part of two right angles*. (It may be noted that  $\tan 2\phi_1 = 2$ ; but that to take out  $\phi_1$  by this equation would require another opening of the tables.)

5. To fix then decisively the *direction* of the *error* of the approximation, and to form with any exactness an estimate of its *amount*, or even to prove quite satisfactorily by *calculation* that any such error *exists*, it becomes necessary to fall back upon arithmetic; and to carry at least the first extractions to several more places of decimals,—although fewer than those which have been actually used in the resumed computation might have sufficed, except for the extreme accuracy aimed at in the resulting



values. For this purpose, it has been thought convenient to introduce eight auxiliary numbers,  $a \dots h$ , which can all be calculated by square roots, and are defined with reference to the recent equations (B), as follows:

$$a = 1 + 2 \tan \phi_1; \quad b = 4 \cos 2\phi_2 \cot \frac{\pi}{5}; \quad c = 2 \cos 2\phi_2 - \cot \frac{\pi}{5};$$

$$d = \sec 2\phi_4; \quad e = \sec 2\phi_5; \quad f = 2 \cos^2 \phi; \quad g = 2 \cos \phi; \quad h = 2 \cos \frac{\phi}{2};$$

or thus with reference to the earlier equations (A):

$$a = \frac{r+r'}{r}; \quad b = \frac{8qr}{p^2}; \quad c = \frac{2(q-r)}{p}; \quad d = \frac{2s+p}{p}; \quad e = \frac{r''}{r};$$

$$f = \frac{t}{r}; \quad g = \frac{v}{r}; \quad h^2 = \frac{2r+v}{r};$$

and respecting which it is to be observed that  $c$ , like the rest, is positive, because it may be put under the form

$$c = \sqrt{\frac{14-2\sqrt{5}}{5}} - \sqrt{\frac{5+2\sqrt{5}}{5}},$$

and  $14-2\sqrt{5} > 5+2\sqrt{5}$ , because  $9 > 4\sqrt{5}$ , or  $9^2 > 4^2 5$ . With these definitions, then, of the numbers  $a \dots h$ , and with the help of the following among other identities,

$$\cos \frac{7\phi}{2} \sec \frac{\phi}{2} = 2 \cos 3\phi - 2 \cos 2\phi + 2 \cos \phi - 1$$

$$= 2(2 \cos \phi - 1) \cos 2\phi - 1,$$

I form *without tables* a system of values as below, the early numbers of which have been computed to several decimals more than are set down.

(C) {	$a^2 = 5$	$a = 2.23606\ 79774\ 99789\ 6964$
	$b^2 = 8 + \frac{72a}{25}$	$b = 3.79998\ 36545\ 96345\ 0138$
	$c^2 = \frac{19}{5} - b$	$c = 0.00404\ 29449\ 23565\ 7641$
	$d^2 = 1 + (2-c)^2$	$d = 2.23245\ 25898\ 01044\ 7849$
	$e^2 = 1 + (5-2a)(d-1)^2$	$e = 1.34230\ 90137\ 74792\ 5831$
	$f^2 = (5-2a)e^2 + 2e - 1$	$f = 1.62349\ 00759\ 24105\ 2470$
	$g^2 = 2f$	$g = 1.80193\ 78878\ 99638\ 5912$
	$h^2 = 2 + g$	$h = 1.94985\ 58633\ 65197\ 2049$
	$\sin \frac{\pi-7\phi}{2} = h \left( (f-1)(g-1) - \frac{1}{2} \right) = +0.00000\ 06134\ 49929\ 1683.$	

Admitting then the known value,

$$\pi = 3.14159\ 26535\ 89793 \dots,$$

or the deduced expression,

$$1'' = \frac{\pi}{648000} = 0.00000\ 48481\ 36811\ 095 \dots,$$

I infer as follows:

$$(D) \begin{cases} \frac{\pi - 7\phi}{2} = + 0''.12653\ 31307\ 822, \\ \frac{\pi}{7} - \phi = + 0''.03615\ 23230\ 806, \\ \phi = 25^\circ\ 42'\ 51''.39241\ 91054\ 91, \end{cases}$$

and think that these twelve decimals of a second, in the value of the angle  $\phi$ , may all be relied on, from the care which has been taken in the calculations.

6. The following is a quite different way, as regards the few last steps, of deducing the same ultimate numerical results. Admitting (comp. Art. 3) the value\*,

$$2 \cos \frac{2\pi}{7} = z = 1.24697\ 96037\ 17467\ 06105,$$

as the positive root, computed by Horner's method, of the cubic equation

$$z^3 + z^2 - 2z - 1 = 0,$$

and employing the lately calculated value  $f$  of  $1 + \cos 2\phi$ , I find by square roots the following sines and cosines, with the same resulting error of the angle  $\phi$  as before:

$$(E) \begin{cases} \sin \frac{\pi}{7} = \frac{1}{2} \sqrt{2-z} = 0.43388\ 37391\ 17558\ 1205; \\ \cos \frac{\pi}{7} = \frac{1}{2} \sqrt{2+z} = 0.90096\ 88679\ 02419\ 1262; \\ \sin \phi = \frac{1}{2} \sqrt{4-2f} = 0.43388\ 35812\ 03469\ 1138; \\ \cos \phi = \frac{1}{2} \sqrt{2f} = \frac{1}{2} g = 0.90096\ 89439\ 49819\ 2956 \\ \sin \left( \frac{\pi}{7} - \phi \right) = + 0.00000\ 01752\ 71408\ 3339; \\ \frac{\pi}{7} - \phi = + 0''.03615\ 23230\ 806. \end{cases}$$

\* Compare a preceding note.

7. If we continue the construction, as Röber did, so as to form an isosceles triangle, say ABC, with  $\phi$  for its vertical angle, and if we content ourselves with thousandths of seconds, the angles of this triangle will be as follows :

$$(F) \begin{cases} A = \phi & = 25^\circ 42' 51'' \cdot 392; \\ B = \frac{\pi - \phi}{2} & = 77^\circ 8' 34'' \cdot 304; \\ C = B & = 77^\circ 8' 34'' \cdot 304; \end{cases}$$

and we see that each base-angle exceeds the triple of the vertical by only about an eighth part of a second, namely by that small angle which occurs first in the system (D), and of which the sine is the last number in the preceding system (C). And if we compare a base-angle of the triangle thus constructed, with

the base-angle  $\frac{3\pi}{7} = 77^\circ 8' 34'' \cdot 2857 \dots$  of the true triangle, in which each angle at the base is triple of the angle at the vertex, we find an error in excess equal nearly to  $0'' \cdot 018$ , or, more exactly,

$$B - \frac{3\pi}{7} = \frac{\pi}{14} - \frac{\phi}{2} = +0'' \cdot 01807\ 61615\ 403,$$

which amounts to *less than a fifty-fifth part of a second*, but of which I conceive that all the thirteen decimals here assigned are correct. And I suppose that no artist would undertake to construct a triangle which should more perfectly, or so perfectly, fulfil the conditions proposed. The *problem*, therefore, of constructing *such a triangle*, and with it the *regular heptagon*, by *right lines and circles only*, has been *practically solved* by that process which Röber believed to have been *known* to the ancient Egyptians, and to have been *employed* by them in the architecture of some of their temples—some *hints*, as he judged, being intentionally preserved in the details of the workmanship, for the purpose of being *recognized*, by the initiated of the time, or by men of a later age.

8. Another way of rendering conceivable the extreme smallness of the practical error of that process, is to imagine a series of seven successive chords inscribed in a circle, according to the construction in question, and to inquire *how near* to the initial point the final point would be. The answer is, that the *last* point would fall *behind the first*, but only by about *half a second* (more exactly by  $0'' \cdot 506$ ). If then we suppose, for illustration, that these chords are *seven successive tunnels*, drawn *eastward* from station to station of the *equator of the earth*, the last tunnel would emerge to the *west* of the first station, but only by about *fifty feet*.



9. My own studies have not been such as to entitle me to express an opinion whether the architectural and geometrical drawings of Röber in connexion with the plan of the temple at Edfu, and his comparisons of the numbers deduced from the *details* of his construction with French measurements previously made, are sufficient to bear out his conclusion, that the process had been anciently used: but I wish that some qualified person would take up the inquiry, which appears to me one of great interest, especially as I see no antecedent improbability in the supposition that the construction in question may have been invented in a very distant age. The geometry which it employs is in no degree more difficult than that of the Fourth Book of Euclid\*; and although I have no conjecture to offer as to what may have suggested the particular process employed, yet it seems to me quite as likely to have been discovered thousands of years ago, perhaps after centuries of tentation, as to have been first found in our own time, which does not generally attach so much importance to the heptagon as a former age may have done, and which perhaps enjoys no special facilities in the search after such a *construction*, although it supplies means of proving, as above, that the proposed solution of the problem is *not mathematically perfect*.

10. If Röber's professional skill as an architect, combined with the circumstance stated of his having previously invented the construction for himself, did really lead him to a correct interpretation† of the plan of the temple at Edfu, which he believed to embody a tradition much older than itself, we are thus admitted to a very curious glimpse, or even a partial view, of the nature and extent, but at the same time the imperfection, of that knowledge of geometry which was possessed, but kept secret, by the ancient priests of Egypt. I say the *imperfection*, on the supposition that the above described construction of the *heptagon*, if known to them at all, was thought by them to be *equal in rigour*, as the elder Röber appears to have thought it to be, to that construction of the *pentagon* which Euclid *may* have learned from them, *rejecting* perhaps, at the same time, the

\* The segment  $p'$  of the side  $p$  of the pentagon, and the fourth proportional  $\frac{r' r''}{r}$  to the three radii, which enter into the equations (A), and of which the latter is the greater segment of the third diameter,  $2r''$ , if this last be cut in extreme and mean ratio, may at first appear to depend on the Sixth Book of Euclid, but will be found to be easily constructible without going beyond the Fourth Book.

† It ought in fairness to be stated that Röber's *interpretation* of Egyptian antiquities included a vast deal more than what is here described, and that he probably considered the *geometrical part* of it to be the *least interesting*, although still, in his view, an essential and *primary element*.

other construction, as being not based on demonstration, and not by him demonstrable, although Euclid may not have *known* it to be, in its result, imperfect. The interest of the speculation stretches indeed back to a still earlier age, and may be connected in imagination with what we read of the "wisdom of the Egyptians." But I trust that I shall be found to have abstained, as I was bound to do, from any expression which could imply an acquaintance of my own with the archæology of Egypt, and that I may at least be pardoned, if not thanked, for having thus submitted, to those who may be disposed to study the subject, a purely mathematical\* discussion, although connected with a question of other than mathematical interest.

W. R. H.

Observatory of Trinity College, Dublin,  
December 22, 1863.

XXV. *Barometrical Formula resulting from the Observations made by Mr. James Glaisher in Eight Balloon-Ascents in 1862.*  
By Count PAUL DE SAINT-ROBERT†.

THE knowledge of the law of decrease of temperature with the altitude is most important, since the determination of heights by the barometer, as well as that of atmospheric refraction, depends upon it.

In the present state of meteorological physics, we know only the general causes which tend to lower the temperature of the air as we recede from the earth's surface we are ignorant of the precise amount of their respective actions.

It is certain that the mean temperature of the air in contact with the earth's surface must be equal to that of this surface, and that that of the superior limit of the atmosphere cannot surpass the temperature at which that fluid loses all its elasticity.

The first condition results from the continual contact of the inferior layer of the atmosphere with the earth's surface.

The second is a condition necessary to the equilibrium of the fluid mass: in fact, if the air at the upper limit of the atmosphere were still expansible, the limiting stratum would dissipate itself into space; after which the immediately inferior

\* Note added during printing.—Some friends of the writer may be glad to know that these long arithmetical calculations have been to him rather a relaxation than a distraction from his more habitual studies, and that there are already in type 672 octavo pages of the 'Elements of Quaternions,' a work which (as he hopes) is rapidly approaching to the stage at which it may be announced for publication.

Observatory, January 19, 1864.

† Communicated by the Author.

stratum being no longer compressed, would dissipate itself in its turn, and successively the whole atmosphere would finish by evanescing. It is by cold that the extreme strata must lose their elasticity and be checked from flying away. If the general equation of dilatability of air,

$$p = \frac{\rho (1 + \alpha\theta)}{\rho_0(1 + \alpha\theta_0)} p_0,$$

which links the pressure  $p$  to the density  $\rho$  and to the temperature  $\theta$ , held good throughout the whole atmosphere, the final condition of no pressure would be given by the temperature

$$\theta = -\frac{1}{\alpha} = -461^{\circ} \cdot 2 \text{ Fahrenheit.}$$

This is the temperature which is considered as the absolute zero in the mechanical theory of heat.

Between the temperature at the earth's surface and the limiting temperature, which prevents the expansion of the external layer of the atmosphere, we know by observation that the temperature of air decreases with the elevation, excepting accidental anomalies sometimes observed in the lower strata, most susceptible of being modified by the contact of the soil.

The law of this decrement, on ascending into the atmosphere, can at present be known only by experience; and the best means of investigating it is by balloon-ascents.

The most important observations on this head hitherto made are Mr. Glaisher's, an account of which has lately appeared\*.

The general conclusion of these remarkable observations is, that the temperature of the air does not decrease uniformly with increase of elevation above the earth's surface, but in a ratio diminishing with the height.

Now the analytical formula universally employed for calculating heights from barometric observations (Laplace's formula) supposes implicitly quite the reverse, viz. that the temperature decreases in an accelerating ratio. Denoting the absolute temperatures (above the absolute zero estimated at  $-461^{\circ} \cdot 2$  F.) of the extremities of a column of air by  $t_0$  and  $t$ , the height of the column by  $x$ , Laplace's formula is grounded on the hypothesis that

$$x = b(t_0^2 - t^2),$$

$b$  being a constant. According to this law, the increment of

\* An Account of Meteorological and Physical Observations in Eight Balloon-Ascents, made at the request of the Committee." By James Glaisher, F.R.S. (From the Report of the British Association for the Advancement of Science for 1862.)



height corresponding to a decline of  $1^{\circ}$  is given by

$$\frac{x}{t_0 - t} = b(t_0 + t);$$

that is to say, by a quantity decreasing with the temperature  $t$ , and therefore with the altitude.

Seeing the incompatibility of the received formula with the observed facts, it has appeared to me worth while to inquire to what formula for computing heights from barometric observations Mr. Glaisher's results lead. In another paper I will endeavour to investigate the formula for atmospheric refraction deducible from the same results.

The decrease of temperature of the air from the earth to the different elevations found by Mr. Glaisher, taking the mean of his eight ascents, is shown in the following Table\* :—

Height above the level of the sea.		Sky partially clear.		Sky cloudy.	
From	To	Decrease of temperature.	Average height for a decline of $1^{\circ}$ .	Decrease of temperature.	Average height for a decline of $1^{\circ}$ .
feet.	feet.		feet.		feet.
0	1,000	7.2	139	4.5	222
	2,000	12.5	160	8.7	230
	3,000	17.1	176	12.8	234
	4,000	20.5	195	16.5	242
	5,000	23.2	211	19.6	255
	6,000	26.0	230		
	7,000	28.8	243		
	8,000	31.5	254		
	9,000	34.1	263		
	10,000	36.7	272		
	11,000	39.3	279		
	12,000	41.9	286		
	13,000	44.4	293		
	14,000	46.6	300		
	15,000	48.7	308		
	16,000	50.8	314		
	17,000	52.7	322		
	18,000	54.5	330		
	19,000	56.3	337		
	20,000	57.8	346		
	21,000	59.1	355		
	22,000	61.4	358		
	23,000	62.4	368		
	24,000	63.7	377		
	25,000	64.8	386		
	26,000	65.8	396		
	27,000	66.8	404		
	28,000	67.7	413		
	29,000	68.5	423		
	30,000	70.0	428		

\* It is perhaps not useless to anticipate an objection that may be made against the manner in which these results were obtained. The heights con-

Looking closely at these results, I find that they may be represented by the expression

$$x = a(t_0 - t) - b(t_0^2 - t^2),$$

$t$  and  $t_0$  being the absolute temperatures of the two extremities of the height  $x$ ,  $a$  and  $b$  two constants. In fact the increase of elevation by every degree of decline of temperature resulting from it is given by

$$\frac{x}{t_0 - t} = a - b(t_0 + t),$$

or

$$\frac{x}{t_0 - t} = a - 2bt_0 + b(t_0 - t).$$

According to this expression, the curve having for abscissa the decrease of temperature  $t_0 - t$ , and for ordinate the elevation corresponding to a decline of  $1^\circ$ , must be a straight line. Now I find that the results of the two last columns being laid down on a diagram, the curve joining them is approximately a straight line which, for a sky partially clear, cuts the axis of ordinates at a distance of about 110 feet from the origin, and is inclined to the axis of abscissæ in an angle whose tangent is nearly 4.2. In a cloudy state of the sky, the straight line cuts the axis at the distance of about 214 feet, and makes an angle whose tangent is 1.8 nearly.

Therefore we have for a partially clear sky

$$a - 2bt_0 = 110, \quad b = 4.2;$$

and for a cloudy sky,

$$a - 2bt_0 = 214, \quad b = 1.8.$$

If with these values of the parameters we calculate the heights corresponding to a decline of  $1^\circ$ , we shall obtain the following Table, showing the difference between the calculated and observed results:—

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tained in the first column of the Table were deduced barometrically by Laplace's formula, which, as we have seen, implies a hypothesis contrary to the real decrement of heat, so that they are faulty; but it must be remarked that the results furnished by the said formula, although not quite correct, do not differ much, particularly for moderate elevations, as will be shown hereafter, from the correct ones.

The unexceptionable method of determining the heights would be to take as a starting-point the relation between the temperature and the barometric pressure given directly by observation throughout the balloon ascent, as is fully explained in Sir John Herschel's 'Meteorology' (art. 27 and following). Yet the method by which the Table was calculated is sufficiently correct for ascertaining if the rate of the decrease of heat is accelerated or retarded; and the general conclusion arrived at, viz. that the decrease is slower the higher we ascend, may be relied upon.

Height above the level of the sea.		Sky partially clear.		Sky cloudy.	
From	To	Calculated average height for a decline of 1°.	Difference between calculated and observed height.	Calculated average height for a decline of 1°.	Difference between calculated and observed height.
feet. 0	feet. 1,000	feet. 140	feet. + 1	feet. 222	feet. 0
	2,000	162	+ 2	230	0
	3,000	182	+ 6	237	+3
	4,000	196	+ 1	244	+2
	5,000	207	- 4	249	-6
	6,000	219	-11		
	7,000	231	-12		
	8,000	242	-12		
	9,000	253	-10		
	10,000	264	- 8		
	11,000	275	- 4		
	12,000	286	0		
	13,000	296	+ 3		
	14,000	306	+ 6		
	15,000	315	+ 7		
	16,000	323	+ 9		
	17,000	331	+ 9		
	18,000	339	+ 9		
	19,000	346	+ 9		
	20,000	353	+ 7		
	21,000	358	+ 3		
	22,000	368	+10		
	23,000	372	+ 4		
	24,000	378	+ 1		
	25,000	382	- 4		
	26,000	386	-10		
	27,000	391	-13		
	28,000	394	-19		
	29,000	398	-25		
	30,000	404	-24		

Further experiments may render it necessary to add other terms to the equation representing the law of decrement. In order to preserve more generality to that expression, I will assume the equation

$$x = a(t_0 - t) - b(t_0^2 - t^2) + c(t_0^3 - t^3) + \&c. \quad (1)$$

as representing the law of decrement of heat.

This being premised, we shall proceed to find the barometrical formula arising from it.

If  $p$  be the pressure of the air of the density  $\rho$  at any height  $x$  where the gravity is  $g$ , it is well known that the equation of equilibrium of a column of air is

$$dp = -g\rho dx.$$

Moreover  $p$  is linked to  $\rho$  by the relation

$$p = h\rho t,$$



$t$  being the absolute temperature of the air, and  $h$  a constant. Hence we have the equation

$$h \frac{dp}{p} = -g \frac{dx}{t},$$

whose integral will give the relation between the pressure  $p$  and the temperature  $t$ , when  $x$  is known in functions of  $t$ .

By equation (1) we have

$$dx = -adt + 2bt \, dt - 3ct^2 \, dt - \&c.,$$

and therefore

$$\frac{h}{g} \cdot \frac{dp}{p} = a \frac{dt}{t} - 2b \, dt + 3ct \, dt + \&c.$$

Integrating, we shall find

$$\frac{h}{g} \log p = a \log t - 2bt + \frac{3}{2} ct^2 + \&c. + \text{constant} :$$

the logarithms are hyperbolic.

Continuing to denote by  $p_0$  and  $t_0$  the pressure and the absolute temperature at the lower station, we have for determining the constant the equation

$$\frac{h}{g} \log p_0 = a \log t_0 - 2bt_0 + \frac{3}{2} ct_0^2 + \&c. + \text{constant}.$$

Thence

$$\frac{h}{g} \log \frac{p_0}{p} = a \log \frac{t_0}{t} - 2b(t_0 - t) + \frac{3}{2} c(t_0^2 - t^2) + \&c. \quad (2)$$

Now as we can determine the parameter  $a$  by the observed temperatures  $t_0$  and  $t$  at the lower and upper stations, we deduce from equation (1)

$$a = \frac{x}{t_0 - t} + b(t_0 + t) - c \frac{t_0^3 - t^3}{t_0 - t} - \&c. ;$$

substituting which in (2), we derive

$$\begin{aligned} x = & \frac{h}{g} \frac{t_0 - t}{\log \frac{t_0}{t}} \log \frac{p_0}{p} - b \left[ t_0^2 - t^2 - 2 \frac{(t_0 - t)^2}{\log \frac{t_0}{t}} \right] \\ & + c \left[ t_0^3 - t^3 - \frac{3}{2} \frac{(t_0 - t)(t_0^2 - t^2)}{\log \frac{t_0}{t}} \right] + \&c. \quad (3) \end{aligned}$$

for the barometrical formula.

According to Mr. Glaisher's results, the value of the parameters  $c$ ,  $d$ , &c. must be very small. Limiting ourselves then to

the first two terms, we have the formula

$$x = \frac{h}{g} \frac{t_0 - t}{\log \frac{t_0}{t}} \log \frac{p_0}{p} - b(t_0 - t) \left[ t_0 + t - 2 \frac{t_0 - t}{\log \frac{t_0}{t}} \right]. \quad (4)$$

for calculating the difference of level  $x$  between two stations where the barometric pression and the temperature are known.

As the term between crotchets is very small, it is convenient to introduce the development of  $\log \frac{t_0}{t}$ , which is

$$\log \frac{t_0}{t} = \frac{2(t_0 - t)}{t_0 + t} + \frac{1}{3} \frac{2(t_0 - t)^3}{(t_0 + t)^3} + \&c.,$$

a very convergent series.

The barometrical formula will then become, neglecting the second power of the fraction  $\frac{t_0 - t}{t_0 + t}$  as very small,

$$x = \frac{h}{g} \frac{t_0 - t}{\log \frac{t_0}{t}} \log \frac{p_0}{p} - \frac{1}{3} b \frac{(t_0 - t)^3}{t_0 + t}.$$

The first term of the value of  $x$  is due to a uniform decrease of temperature with the altitude, and the second is due to the retarding ratio of that decrease.

We must observe that if we expand  $\log \frac{t_0}{t}$  in the first term of  $x$ , we obtain

$$x = \frac{h}{g} \left( \frac{t_0 + t}{2} \right) \log \frac{p_0}{p} - \frac{h}{g} \left( \frac{t_0 + t}{2} \right) \log \frac{p_0}{p} \cdot \frac{1}{3} \left( \frac{t_0 - t}{t_0 + t} \right)^2 - \frac{1}{3} b \frac{(t_0 - t)^3}{t_0 + t},$$

neglecting only the third power of  $\frac{t_0 - t}{t_0 + t}$ , because the quantity by which it is multiplied is much larger than the coefficient  $b$ .

The first term is the well-known formula given by Laplace. Therefore if we denote by  $X$  the height furnished by Laplace's formula, the correction to be applied to it, to take into account the retarding ratio of decrement of heat, is expressed by

$$- \frac{1}{3} X \left( \frac{t_0 - t}{t_0 + t} \right)^2 - \frac{1}{3} b \frac{(t_0 - t)^3}{t_0 + t}.$$

This correction is very small for small differences of the temperatures of the two stations—that is to say, for small differences of level—but becomes of consequence for considerable differences of temperature, and therefore of level.

For instance, if we take

$$t_0 - t = 50^{\circ} \cdot 8 \text{ Fahr.},$$

a decrease of temperature corresponding to a height of 16,000 feet (not far from that of Mont Blanc), the correction would be 197 feet (supposing, for the sake of illustration,  $t_0 = 525^{\circ}$ ,  $b = 4 \cdot 2$ ). For

$$t_0 - t = 70^{\circ},$$

corresponding to a height of 30,000 feet, the correction is 541 feet.

Whatever be the value of  $b$  in (1), provided it is positive, the correction to be applied to Laplace's formula is always negative. After Mr. Glaisher's observations it is impossible to suppose  $b$  negative; therefore all the heights calculated in balloon-ascents must be diminished.

It is not certain that the same correction is to be applied to the altitudes of mountains deduced from barometric observations; for we must admit that, as a general rule, the temperatures of the extremities of a column of air remote from any ground, will not be the same as that of the air at two stations on the flanks of a mountain, on account of the variable mode in which the surface of the latter is heated and imparts its temperature to the air in contact.

Simultaneous thermometrical observations made on the top of mountains and in the air at the same level by means of captive balloons would tend to elucidate that question.

So far as observation has hitherto gone, the altitude of a station on a mountain, as derived from barometric observations, varies with the hour of the day. From about 1 p.m. till 3 in the morning the altitude goes on diminishing; and then it increases from that hour up to the moment of the maximum, which is usually about an hour after the sun passes the meridian of the place\*.

Observations of the same kind should be repeated in many places varying in exposition and climate. It is desirable that the attention of alpine travellers should be directed to this subject.

Much may be expected from the joint exertion of many well-informed persons who, now that alpine clubs are spreading everywhere, frequent the higher regions of the Alps and other mountain-districts.

\* See Prof. Piazzzi Smyth, "Astronomical Experiment on the Peak of Teneriffe," in the Philosophical Transactions of the Royal Society of London, 1859. E. Plantamour, "Mesures Hypsométriques dans les Alpes," *Mém. de la Soc. de Physique et d'Histoire Naturelle de Genève*, 1860. Charles Martins, "Sur l'influence de la distance et la correction horaire des différences de Niveau," *Archives Scientifiques de la Bibliothèque Universelle de Genève*, 1860.



Here is an opportunity for persons of moderate scientific attainments, who will carry with them good instruments and observe them with care, to contribute to the improvement of a department of science which is yet far from having attained to the desirable degree of perfection.

Pisa, January 1864.

## XXVI. *Notices respecting New Books.*

*The Mathematical and other Writings of Robert Leslie Ellis, M.A., late Fellow of Trinity College, Cambridge. Edited by W. WALTON, M.A. With a Biographical Memoir, by the Very Rev. HARVEY GOODWIN, D.D., Dean of Ely. Cambridge, 1863. 8vo, pp. 427.*

THIS volume is of a memorial rather than of a strictly scientific character. It contains in a collected form the miscellaneous memoirs and notes of a man of rare promise, but who was taken away prematurely and under peculiarly painful circumstances. Mr. Ellis, the youngest of a family of six children, was born at Bath, August 25, 1817. He was carefully educated at home, and it may be mentioned that one of his mathematical tutors was the late Mr. T. S. Davies. He entered as a pensioner at Trinity College, Cambridge, and went into residence in October 1836. He appears to have brought a large stock of mathematical knowledge with him to Cambridge, and pursued his studies there without the aid of a private tutor until his last year, when he read with Mr. Hopkins, who remembers that he possessed an extent and definiteness of acquirement and a maturity of thought so unusual "that I could hardly conceive when he could have been a boy." He took his B.A. degree in January 1840, when he was Senior Wrangler, Dr. Goodwin being second in the same year. In the following October he was elected Fellow of Trinity College, and continued to hold his fellowship till 1849, when, as he did not take orders, he vacated it in due course. He inherited from his mother a highly nervous constitution and extremely feeble health—circumstances which prevented his entering any active profession. He was, it is true, called to the bar; but though he had paid great attention to the principles of Civil Law, he did not study long with a view to practice. In like manner he once thought of attempting to enter Parliament, and his name was publicly brought forward as a possible candidate for the representation of Bath, but was withdrawn on the score of his ill health. Neither ill health, however, nor nervous constitution impaired his powers as a student: he seems to have been in an eminent degree capable of acquiring and retaining a knowledge of many distinct subjects; and though his mathematical power was great, it is said by his friends to have been no greater than several of his other powers—an opinion which his remains bear out. He resided mostly at Cambridge, pursuing various studies, not taking much part in the work of the place beyond being Moderator in 1844, and Examiner in 1845 for the Senate House Examination. He assisted the late Mr. D. F. Gre-

gory in starting the '*Cambridge Mathematical Journal*,' and edited the third and fourth volumes of that periodical. In conjunction with Mr. Spedding and Mr. D. D. Heath, he undertook the task of bringing out a new edition of Lord Bacon's works; the portion comprising the philosophical works fell to his share. In the autumn of 1849, after vacating his fellowship, he left England, apparently with the intention of ultimately settling down in a warmer climate. After a short stay at Nice he went on to Mentone, where he slept in a damp bed; on the evening of the next day but one he was seized by a rheumatic fever. For some days the attack seemed fatal, but unhappily for him it was otherwise ordered. He continued for nearly ten years, literally dying by inches. All this time his nervous system seems to have been undergoing a process of slow disorganization. He was a cripple from the time of the fever, and his remaining bodily powers failed one by one during the ensuing years. His mental powers, however, continued bright to the end; and as he found some relief in the society of intellectual friends, he returned to the neighbourhood of Cambridge, where he passed the last six years of his life. His religious feelings, originally strong, were deepened by his sufferings, which he bore very meekly; and though longing for death, he was content to wait till it came. Dr. Goodwin has recorded many interesting recollections of his last years; but on them we must not dwell. He was, it is said, a man of somewhat reserved manners, but in the company of friends showed great conversational powers; he seems to have been eminently a "full man," and what he had to say was expressed in singularly clear and appropriate diction. The portrait prefixed to the volume is stated by Mr. Walton to be copied from an admirable likeness; to those who remember Mr. Ellis it may seem that it fails to render adequately the palid hue of sickliness with which his face was marked even in the days of his comparatively good health. This paleness was in strong contrast with the extreme blackness of his hair; and the general impression of sickliness was heightened by a feebleness of gait quite remarkable in a man so young. We may also notice that the artist has not rendered, or rather has disguised the receding character of the lower part of his face, and the thinness of his face generally. His countenance was marked by rare intellectual beauty; and this is adequately rendered. Resemblance of character and feature between persons in no wise connected is always to be understood with great abatement; that premised, we may say that he seems to us to have borne in both respects considerable likeness to Pascal: the early tendency towards mathematical studies, the clearness of expression and subtilty of thought, the deep religious feeling, the early closing in of a melancholy latter life, were the same in both. Differences in abundance there were—a great difference in power and genius; but perhaps the most striking point of difference is that suggested by Mr. Ellis's own words, "He who loves truth earnestly will be slow to believe that the cause of truth is to be served by irony"\*.

\* General Preface, p. 65, Bacon's Works, vol. i.

We have dwelt so long upon the man, that our remarks upon his writings must be brief. The title-page of the present volume would, we think, justify the expectation that it contains at least all Mr. Ellis's printed works; this, however, is not the case. That part of his writings which is contained in the edition of Bacon's works is not given here, an omission not to be wondered at, but to be regretted; at least we should be glad to see the General Preface to Bacon's philosophical works printed separately. We quite agree with Dr. Goodwin that it is the most valuable of his writings. We may add that it contains a singularly able exposition of the more difficult points of Bacon's philosophy, and particularly of the meaning of his chief technical terms.

The most important article in the present volume is perhaps the "Report on the Recent Progress of Analysis" (pp. 238-323), presented to the British Association in 1846. It contains an account of the progress made in the Theory of Elliptic Functions and the Comparison of Transcendentals in the twenty years preceding the writing of the Report. Next in prominence, if not in importance, is a series of articles on the Theory of Probabilities, and on the Method of Least Squares. These articles Dr. Goodwin considers to represent Mr. Ellis's special tendency in regard to mathematics:—"He delighted to discuss the principles of investigations already known, to trace the history of processes, to examine the philosophy of a subject, to hunt up its literature, or to simplify its treatment." They also illustrate the influence which the writings of Dr. Whewell exerted on the formation of his philosophical views, an influence which may be noticed in several of his other writings. The first, "On the Foundation of the Theory of Probabilities," is devoted to an attempt to show that James Bernoulli's theorem, viz. that in a long series of trials events tend to recur in numbers proportioned to their individual probabilities, is not really susceptible of proof, but is a fundamental and self-evident principle. He does not seem to have been quite satisfied with this article; and eight years after, in order to strengthen his view, he wrote a sort of Appendix to it, entitled "Remarks on the Fundamental Principle," &c. The next is a review of the methods that have been proposed for proving the principle of least squares; this is followed by remarks on "an alleged proof" of the same principle "contained in a late Number of the Edinburgh Review." In one respect the article, which is written with great spirit, is remarkable, as he composed it twelve months after the commencement of his last illness. The author of "the alleged proof" was Sir John Herschel, who has since republished the article; and we surmise that the short note on page 399 of Sir John Herschel's 'Essays' is intended to meet one of Mr. Ellis's objections. The last is the solution of a detached problem in probability.

The remainder of the mathematical part of the volume contains about thirty-five articles, mostly devoted to the solution of isolated questions in several of the higher branches of the science; some of them were dictated to an amanuensis during his last illness. These exemplify a power, which Mr. Ellis possessed in great perfection, of



following a long train of analytical reasoning without so much as seeing the symbols. A few essays and remarks on other subjects are inserted, partly on account of their intrinsic merit, partly, we suppose, with a view to exhibiting the extensive range of Mr. Ellis's learning. We may particularize the following:—an essay "On Roman Aqueducts," written for the purpose of determining the amount of hydraulic knowledge exhibited in the arrangements made for distributing the water, so far as they are known. "Some Thoughts on Comparative Metrology"—an attempt to determine historically the connexion between the measures of land used by different European nations. It would appear from internal evidence that this essay could not have been composed before the end of 1852. If this circumstance is borne in mind, it may be justly thought that the learning and out-of-the-way knowledge displayed in the course of it render its composition one of the most remarkable instances on record of mental powers continuing in unimpaired vigour in the midst of bodily decay; for Mr. Ellis's illness had now been going on for more than three years. We may just name "Remarks on certain Words in Diez's Etymological Dictionary of the Romance Languages," and "Some Thoughts on the formation of a Chinese Dictionary." The last article in the volume contains remarks "On the Course of Mathematical Studies," being evidence laid before the "Cambridge University Commission." Want of space forbids us to make any remarks on this article, which suggests more questions than one well worth discussion, and would well repay careful perusal.

We must here close our notice of this mournful volume, the contents of which we have attempted rather to describe than criticise. Had we taken the latter course, it might have been necessary to raise objections to some of Mr. Ellis's views. But of this there was no need. The interest of the volume does not lie in its contents considered merely as matters of science, but rather as the memorial of a singularly gifted man cut off before his promise ripened into performance. The volume does not tell what the world has gained by its author's life, but rather what it has lost by his death.

We must add that Mr. Walton has discharged his duty as editor very faithfully. So far as the printing, and the form and appearance of the volume are concerned, there is little or nothing to be desired.

## XXVII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 73.]

April 30, 1863.—Major-General Sabine, President, in the Chair.

THE following communications were read:—

"On Spectrum Analysis, with a Description of a large Spectroscope having nine Prisms, and Achromatic Telescopes of two feet focal power." By John P. Gassiot, F.R.S.

The author, after briefly alluding to the discoveries of Fox Talbot, Wheatstone, Foucault, Kirchhoff, and Bunsen, and the im-



portance of spectrum analysis, states that among the numerous spectroscopes which were exhibited in the International Exhibition of 1862, there was one which had been specially constructed by Messrs. Spencer, Browning, and Co., philosophical instrument makers in London, which at the time excited considerable attention. This spectroscope had two prisms, with a magnifying power of 40, its definition being remarkably clear.

The skill evinced by Mr. Browning in the construction of this instrument induced the author to have one made in which still better effects might be produced, by multiplying the number of prisms and increasing the magnifying power, with the necessary precaution to avoid as much as possible loss of light. After a few preliminary trials, it was finally arranged to use nine prisms, which is the number that can be applied with this instrument, although the arrangements are such as to allow the whole or any less number to be used with the utmost facility.

Verniers and micrometer screws are attached to the knife-edges of the slit through which the light to be observed is admitted to the collimator and to the telescope, also to the large circle of the instrument; these enable the observer to note the exact position of the lines observed in the spectrum from whatever source it is obtained, and thus enable him to repeat and verify previous results with the utmost exactitude.

When two small prisms, one refracting and the other reflecting, are fixed outside the knife-edge slit, spectra obtained from three separate sources can be simultaneously examined; and an illuminated micrometer scale enables the observer to note the precise relative position of the lines in the three spectra without reference to or reading off from the verniers. By this arrangement a most interesting spectacle may be obtained, showing in the uppermost portion of the field of view the spectrum of thallium, strontium, or lithium, ignited in the flame of a Bunsen's gas-burner; in the centre of the field the spectrum of the same substance in the oxyhydrogen blowpipe, and at the bottom one in the voltaic arc; each successive spectrum there exhibits an increased number of lines.

With this spectroscope the author has ascertained that the green line of thallium, so celebrated for its integrity, and hitherto believed to coincide with one of the lines in the spectrum of baryta, does not so coincide; for by employing the nine prisms with a power of 80 on the telescope, the thallium line is clearly seen to occupy a dark space in the baryta spectrum, close by the side of the bright line with which it was supposed to coincide.

A range of prisms is adapted to the telescope, the highest of which, when used in conjunction with the amplifying lens, gives a power of 110 with good definition.

The author states that the results already obtained by this instrument have been so satisfactory as to leave him no cause to regret the time that has been devoted to, or the expense that has been incurred in the construction of this truly beautiful apparatus.

The Bakerian Lecture.—“On the Direct Correlation of Mechanical and Chemical Forces.” By Henry Clifton Sorby, F.R.S.

Perhaps it may be thought somewhat strange that a geologist should undertake such a subject as the correlation of forces; but the very fact of my being a geologist has led to the investigations of which I now purpose to give a short preliminary account. In studying general chemical and physical geology, and especially in examining the microscopical structure of rocks, I have for a number of years been greatly perplexed with a class of facts which pointed both to a mechanical and to a chemical origin. At first I attributed them either to a mechanical or a chemical action, or to the two combined; but in most cases no satisfactory explanation could be given. At length, however, facts turned up which altogether precluded any supposition not involving direct correlation; for they most clearly indicated that mechanical force had been resolved into chemical action in the same way as, under other circumstances, it may be resolved into heat, electricity, or any other modification of force, as so ably described by Grove in his work ‘On the Correlation of Physical Forces.’

The effect of pressure on the solubility of salts has already been made the subject of speculation and experiment\*, and a considerable number of facts have been described, showing that pressure will more or less influence such chemical actions as are accompanied by an evolution of gas, so that it may cause a compound to be permanent which otherwise would be decomposed†; but the results were for the most part so indefinite and unconnected, or of such a character, that Mr. Grove does not allude to the *direct* production of chemical action from mechanical force. That this is, however, extremely probable will be evident to all who have considered the manner in which the various physical forces are correlated; for if mechanical force can be produced by chemical action, why should not the converse be true? In this paper I shall endeavour to show that such is really the fact, and that in some cases the mechanical equivalent of the chemical force may be determined.

In order to obtain the necessary great pressure, I have made use of a modification of the method employed by Bunsen; but instead of filling the tubes at the ordinary temperature of the atmosphere and then gently heating them for several hours, I in the first instance filled them at a temperature  $10^{\circ}$  or  $20^{\circ}$  C. lower, so that when finally sealed up they contained considerably more liquid than they could hold without pressure at the ordinary temperature of the atmosphere

\* Perkins, Ann. de Chim. et de Phys. vol. xxiii. p. 410. Sartorius von Waltershausen, Göttinger Studien, 1857. Bunsen, Ann. der Chem. und Pharm. 1848, vol. lxx. p. 70. Favre, Comptes Rendus, vol. li. p. 1027. Thomson, Proc. Roy. Soc. vol. xi. p. 473 (1861).

† Sir James Hall, Trans. Roy. Soc. Edinb. 1812, vol. vi. p. 71. Wöhler, Ann. der Chem. und Pharm. vol. xxxiii. p. 125. Babinet, Ann. de Chim. et de Phys. (2) vol. xxxvii. p. 183. Lothar Meyer, Pogg. Ann. vol. civ. p. 189. Beketoff, Comptes Rendus, vol. xlviii. p. 442. Gassiot, Brit. Assoc. Report, 1853, p. 39. Favre, Comptes Rendus, vol. li. p. 1027. Berthelot et Péan de Saint-Gilles, L’Institut, 1862, p. 257. Gmelin’s Handbook of Chemistry, published by the Cavendish Society, vol. ii. p. 293.

at the time being; and thus, by its tendency to expand, this liquid and anything enclosed in the tube were subjected to a very great pressure. By keeping the tubes in various parts of the house, according as the weather varied, I have been able to maintain for several weeks or even months a pressure of, for instance, about 100 atmospheres, as measured by means of a capillary-tube pressure-gauge enclosed within the larger tube. Since in all cases I had a second tube which from first to last was treated precisely like the other, pressure excepted, I have been able to determine the effect produced by the pressure with very considerable accuracy—at all events so as to leave no doubt whatever about the general facts. At the same time I wish it to be understood that the results described below must be looked upon only as approximations to the truth.

I will first call attention to the well-known influence of pressure on the fusing-point of various substances, since it is a connecting link between well-established facts and those I am about to describe. Bunsen\* and Hopkins† have shown that substances which expand when fused have their point of fusion raised by mechanical pressure; that is to say, since mechanical force must be overcome in melting, the tendency to melt must be increased by heat before that opposition can be overcome; and the pressure required to keep them solid at any temperature above their natural point of fusion may be looked upon as the mechanical representative of the force with which they tend to fuse at that temperature. Professor W. Thomson‡ has shown that, on the contrary, water, which expands in freezing, has its point of fusion lowered by pressure; that is to say, since mechanical force must be overcome in crystallizing, crystallization will not take place under increased pressure unless the force of crystalline polarity be increased by reducing the temperature. Thus, calculating from his experiments and from the known latent heat of ice, and assuming that no heat is gained or lost by contact with external objects, if we had 1 part of ice and 100 of water at 0° C., and then applied a pressure of 103 atmospheres, the ice would, as it were, dissolve in the water, the whole would become liquid, and the temperature be reduced to -792° C.; or, in other terms, at that temperature the tendency to crystallize is exactly counterbalanced by that pressure.

Now I find that similar principles hold true with respect to the solubility of salts in water. If, when they dissolve, the total bulk increases, pressure reduces their solubility; whereas if the bulk decreases, pressure makes them more soluble; in other words, solution or crystallization is impeded by pressure according as mechanical force must be overcome in dissolving or in crystallizing.

Various authors have written on the volume with which salts enter into solution§; but since the subject before us requires a different

\* Pogg. Ann. 1850, vol. lxxxi. p. 562.

† British Association Report, 1854, p. 57.

‡ Trans. Roy. Soc. Edinb. vol. xvi. p. 575.

§ Playfair, Chem. Soc. Quart. Journ. vol. i. p. 139. Michel and Krafft, Ann. de Chim. 3 sér. vol. xli. p. 471. Schiff, Ann. der Chemie, vol. cix. p. 325; vol. cxi. p. 68; vol. cxiii. p. 349. Gerlach, Specifische Gewichte der Salzlosungen, &c.,



class of facts to be taken into account, I shall base my conclusions on my own experiments. The volume with which salts exist when in solution, assuming that of the water to remain unchanged, varies greatly in the case of different salts, and also according to the amount in solution and the temperature. Thus, taking sal-ammoniac as an example, when there are 3 per cent. in solution the volume is as if it expanded 3.40 per cent. on dissolving; whereas when 25.55 per cent. are in solution, the expansion is 11.36 per cent.; and when nearly concentrated at about 13° C., an additional quantity expands on dissolving 15.78 per cent. In by far the greater number of cases, however, there is a contraction on dissolving, and the amount gradually diminishes for each additional quantity entering into solution, so that the mean result is very different from what occurs when the solution is dilute or is nearly saturated. It is this contraction or expansion when a small additional quantity is dissolved in a nearly concentrated solution that must be taken into account in the following calculations.

In determining the influence of pressure on the solution of salts, I found it requisite to adopt somewhat different methods according to the peculiarities of the salts. In some cases I sealed up in a saturated solution portions of the salt in clean, solid crystals, and determined the effect due to pressure from their loss in weight; whereas in other cases I sealed up solutions containing more salt than could be dissolved at the temperature at which the experiments were made, and determined the effect of pressure from the difference in the weight of the crystals deposited; being of course careful to make allowance for any difference in the amount of solution in the tube with pressure and in that without, and to avoid any error that might be produced by a different temperature. In all cases I have had a tube with pressure and another without, treated from first to last in precisely the same manner, and kept at exactly the same temperature, so that pressure was the only difference; and usually the effect was so well marked that there was no doubt about the result. In the case of chloride of sodium, solution goes on so slowly, and the mechanical equivalent of the force of crystallization is so great, that if pressure had been applied for only a few hours one might have concluded, with Bunsen, that pressure has no influence on solubility; but, by maintaining it for a week or more, there was no difficulty whatever in perceiving that a solution which was quite saturated without pressure, dissolved more under a pressure of about 100 atmospheres.

The solubility of a salt in water appears to me to result from a kind of affinity which decreases in force as the amount of salt in solution increases. This affinity is opposed by the crystalline polarity of the salt; and when the two forces are equal, the solution is exactly saturated. As is well known, a change in temperature alters this equilibrium; and, according to my experiments, mechanical pressure rela-

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1859. Tissier, *L'Institut*, 1859, p. 158; 1860, p. 281. Kremers, *Pogg. Ann.* vol. lxxxv. pp. 37 and 246; vol. xciv. p. 87; vol. xcv. p. 110; vol. xcvi. p. 39; vol. cv. p. 360; vol. cviii. p. 115. Billet, *Thèse de Chemie*.



tively increases one or other of these opposing forces, according to the mechanical relations of the salt in dissolving. At all events in the case of chloride of sodium the extra quantity dissolved under pressure varies directly with it for such pressures as glass tubes will resist, in the same manner as, according to Thomson's experiments, the fusing-point of ice is reduced. Thus I found that for a pressure of  $49\frac{1}{2}$  atmospheres the extra solubility was  $\cdot 176$  per cent., and for 121 atmospheres  $\cdot 431$ , which are almost exactly in the same ratio. Hence, if  $S$  be the amount soluble without pressure, under a pressure of  $p$  atmospheres the solubility at the same temperature would be  $S + ps$ , where the values of  $S$  and  $s$  are independent, and vary for different temperatures and different salts. Future experiments may perhaps show that this conclusion should be modified; but yet it will be well to adopt it provisionally, in order to compare together the mechanical relations of different salts which otherwise would not be so intelligible.

According to Michel and Krafft\* and to Schiff†, sal-ammoniac is the only salt known for certain to occupy more space in solution than when crystallized. Hence under pressure mechanical force must be overcome in dissolving, and experiment shows that, on this account, the relative force of crystalline polarity is increased and the solubility decreased. This is the reverse of what results from an elevation of the temperature, so that the effect cannot be due to heat generated by the pressure, but must be the direct consequent of pressure. Calculating from an experiment where the pressure was 164 atmospheres, which gave a decreased solubility of 1.045 per cent. of the whole salt in solution, a pressure of 100 atmospheres would cause  $\cdot 637$  per cent. less to be dissolved than is soluble at  $20^{\circ}$  C. without pressure, and the pressure requisite to reduce the solubility to the extent of 1 per cent. would be 157 atmospheres. Expressing this fact in other words, we may say that a pressure of 157 atmospheres is the mechanical force with which the salt tends to dissolve in a solution containing 1 per cent. less than can dissolve at the same temperature without pressure, because the two forces exactly counterbalance one another. In a still more dilute solution the force would of course be still greater, in accordance with the fact of a greater pressure being necessary to prevent the salt from being dissolved. Supposing then that we had a solution a trifle more dilute than that just named, and in such indefinitely large quantity that a cubic inch of the salt could dissolve in it and yet produce no sensible change in its strength, so that from first to last it might be considered to dissolve under a pressure of 157 atmospheres, and also supposing that it was rigidly enclosed on all sides but one, so that the whole expansion must take place in one direction over an area of one square inch, since on dissolving there is an increase in bulk from 100 to 115.78, the solution of this cubic inch would, as it were, raise 2355 lbs. through the space of  $\cdot 1578$  inch. This is mechanically the same as  $371\frac{1}{2}$  lbs. raised 1 foot, or, the specific gravity of the salt being 1.53, the same as 171 times the weight of the salt itself raised

\* Ann. de Chim. 3 sér. vol. xli. p. 471.

† Ann. der Chemie, vol. cix. p. 325; vol. cxiii. p. 329.

1 metre. Since it involves no arbitrary unit but the metre, I shall adopt the last expression as the measure of the total amount of mechanical work done by the solution of salts which expand in dissolving, and which may conversely be looked upon as the measure of the mechanical force rendered latent and, as it were, expended in the act of crystallization when crystals are deposited. The value of this mechanical equivalent of course varies with the strength of the solution, as already remarked.

In the case of salts which occupy less space when dissolved than when solid, pressure, like an increase in temperature, causes them to be more soluble; mechanical force is lost when they dissolve, and is, as it were, expended in giving rise to solution. When water thus containing more of a salt than could otherwise be dissolved at the same temperature is just saturated under any given pressure, the amount of pressure represents the force of crystalline polarity tending to cause the salt to be deposited in a crystalline form, but which is exactly counterbalanced by that pressure. I will not give the details for each salt, but subjoin a Table of the results at which I have arrived for such as illustrate particular points of interest, the calculations being all made in accordance with the principles already described. I also give them in the case of water, calculated from Thomson's experiments, assuming that, when ice melts and mixes with water, it may be looked upon as dissolving in it; and, as will be seen, the mechanical force thus deduced is of the same general order of magnitude as that generated by the crystallization of salts.

	I.	II.	III.	IV.	V.
1. Chloride of Sodium. . . .	13.57	97	.407	.419	157
2. Sulphate of Copper. . . .	4.83	60	1.910	3.183	7
3. Ferridcyanide of Potas- sium . . . . .	2.51	86	.288	.335	42
4. Sulphate of Potash. . . .	31.21	63	1.840	2.914	42
5. Ferrocyanide of Potas- sium . . . . .	8.90	66	1.640	2.485	20
6. Water . . . . .	8.93	..	..	.991	106

Nos. 2 and 5 are calculated as hydrated crystals.

Column I. gives the expansion of each salt in crystallizing from a nearly saturated solution in water, the volume in a crystalline state being taken at 100.

Column II. gives the actual pressure in atmospheres in the experiment.

Column III. gives the increased solubility due to the pressure given in column II., the total amount of salt dissolved without pressure being taken at 100.

Column IV. gives the increase in solubility that would be produced by a pressure of 100 atmospheres, as calculated in accordance with the principles already described, the same unit being taken as in column III.

Column V. gives the value of the mechanical work that could be done, or, so to speak, the amount of mechanical force set free when

the various substances crystallize from a solution containing 1 per cent. more than would be dissolved without pressure, as measured by the number of times its own weight which any unit of the various salts could raise to the height of 1 metre in the act of crystallization. Conversely, it is the amount of mechanical force which becomes latent in the act of solution; and in the case of a still more supersaturated solution it would be greater, and *vice versa*, in accordance with the fact of the increased solubility varying with the pressure.

On comparing together the various salts, it will be seen that their properties vary very considerably. Thus, under the same pressure, the extra quantity of sulphate of copper dissolved is nearly ten times that of ferridcyanide of potassium. The mechanical equivalents vary even more, that of chloride of sodium being  $22\frac{1}{2}$  times greater than that of sulphate of copper. On the contrary, the mechanical equivalents of ferridcyanide of potassium and sulphate of potash are the same; but, under equal pressures, the extra quantity of the latter dissolved is nearly nine times as great, owing to the difference in the amount of expansion in crystallizing. This latter is, however, nearly the same for water and ferrocyanide of potassium, whilst, under the same pressure, the extra quantity of that salt dissolved is  $2\frac{1}{2}$  times that of ice, in consequence of the much greater mechanical equivalent of the ice. It appears to me that we may provisionally conclude that the increased solubility due to pressure varies directly with the change of volume, and inversely with the mechanical equivalent of the force of crystalline polarity, so that, if  $S$  be the total amount of salt which dissolves without pressure,  $c$  be some function of the change in volume in dissolving, and  $m$  some function of the mechanical equivalent of the force of crystalline polarity, the solubility, at the same temperature, under a pressure of  $p$  atmospheres would be  $S + \frac{p \cdot c}{m}$ . If the salt be one that expands on dissolv-

ing,  $c$  of course is negative, and therefore under pressure the solubility becomes  $S - \frac{p \cdot c}{m}$ ; that is to say, it is diminished, as proved by ex-

periments with sal-ammoniac. If no change in volume took place, we may, I think, also conclude that pressure would not in any way increase or decrease the solubility of a salt. Moreover, since, when a solution is just saturated, the force with which the salt tends to crystallize is equal to that with which it tends to dissolve, their mechanical equivalents must be equal and opposite. Hence we may perhaps conclude that, other circumstances being the same, the mechanical equivalent of a salt like chloride of sodium, which so readily attracts moisture, would be greater than that of one like sulphate of copper, which so readily loses even its water of crystallization; and thus also the relative influence of equal amounts of pressure would be very different, as is confirmed by experiment in the case of these and some other salts.

The facts I have described, therefore, show that there is a direct



correlation between mechanical force and the forces of crystallization and solution. According to some chemists, the latter is an instance of real combination; but, whatever views be entertained respecting its nature, we cannot, I think, deny that the force represents some modification of chemical affinity, or is at all events most closely allied to it. In comparison with some kinds of affinity, it may indeed be, and probably is, weak; but yet, as I have shown, it sometimes has a very considerable mechanical equivalent, even when nearly counterbalanced by an opposite force; and since such pressures as glass tubes will resist have no very great influence on what we may perhaps consider a weak affinity, we cannot expect that any pressure at our command would have much influence on strong affinities. I have, however, succeeded in obtaining some results which apparently show that pressure influences undoubtedly chemical changes taking place slowly, and therefore probably due to weak, or nearly counterbalanced, affinities.

The method adopted in this part of the inquiry was to seal up some solid substance in a solution which gives rise to a slow double decomposition, taking great care to have in the tube with pressure, and in that without, pieces cut so as to be of the same size and form, and a solution of the same character, so that, with the exception of pressure, all the conditions were the same. Possibly I may be so fortunate as to discover some case where the affinity is so weak that pressure may determine whether decomposition go forward or not, of which fact the structure of metamorphic rocks furnishes examples; but hitherto I have only been able to prove that pressure modifies the *rate* at which the action takes place. This branch of the inquiry is, however, beset with many difficulties, for the change in volume produced by double decomposition is small, and its determination involves several complicated questions. The volume of the solids is easily determined; but that of the salts in solution is not the same when other salts are present as when they are dissolved in pure water, and varies much according to the strength of the solution and the nature of the salts; and many points are still so obscure, that I shall only give two cases by way of example.

When a portion of Witherite is enclosed in a tube with a strong solution of protochloride of iron, there is a slow decomposition into chloride of barium, which is dissolved, and carbonate of iron, which remains firmly attached to the Witherite, and would ultimately give rise to an excellent pseudomorph. The best conclusion at which I have been able to arrive is, that there is in this change an increase in volume equal to about 10·7 per cent. of the Witherite altered, so that, under pressure, mechanical force must be overcome. In an experiment where everything went on in a very satisfactory manner, the pressure was maintained for three months at from 80 to 100 atmospheres, and for one month was under 80 atmospheres, so that, on an average, it was about 80 atmospheres; and I found that the amount of chemical change was 21·7 per cent. less than when, all other circumstances having been the same, there had been no pressure—thus clearly showing that pressure had, as it were, diminished



the force of chemical affinity. If then one cubic inch had been altered under this pressure, it would have overcome a mechanical force equal to that required to raise 1200 lbs. through the space of  $\cdot 107$  inch, which is equivalent to raising twenty-one times its own weight to the height of 1 metre; and under the same circumstances  $1\cdot 278$  cubic inch would have been altered when no such mechanical force had to be overcome. Supposing then that in both cases the total energy at work was the same, but in one was altogether expended in producing a chemical result, and in the other in producing partly a chemical and partly a mechanical effect, we may say that the force which gives rise to the purely chemical change, taking place at a particular rate, is equal to that which gives rise to this chemical effect, taking place at  $\cdot 783$  of that rate, and to a mechanical effect equal to the force required to raise in the same space of time  $34\cdot 87$  times the weight of the Witherite altered to the height of 1 metre. Supposing also that the power of chemical force varies as the rate at which it gives rise to a chemical change, in the same manner as the power of a mechanical force varies as the velocity of motion imparted by it, we may perhaps conclude that this mechanical force is equal to  $\cdot 217$  of the chemical force, and that the whole energy of the chemical action under the conditions of the experiment was equal to the mechanical power required to raise in the same period of time 160 times the weight of the Witherite altered to the height of 1 metre. If these principles are correct, a pressure of more than 370 atmospheres would have entirely counterbalanced the force of chemical affinity, since to produce any chemical change it would then have had to overcome a greater force than it possessed. This is so great a pressure that I fear it will be difficult to prove the deduction by experiment; and until some such case can be found, capable of being verified, these calculations must be considered as little more than suggestions, which future investigations may confirm or disprove.

When calcite is sealed up in a mixed and rather strong solution of chloride of sodium and sulphate of copper, slow double decomposition gives rise to malachite, sulphate of lime, and carbonic acid; and though this case is extremely complicated, and it is very difficult to determine what would be the change in volume, yet, so far as I am able to make out, until the solution becomes saturated with sulphate of lime, there is a decrease in volume equal to about 8 per cent. of that of the calcite altered, so that, under pressure, mechanical force is the very reverse of being opposed to the chemical change. Three experiments, all indicating the same fact, and in which, on an average, the pressure was about 90 atmospheres for two weeks, show that, as a mean of the whole, the amount of chemical change was 17 per cent. more with the pressure than without; thus proving that pressure had, as it were, increased the force of chemical affinity. Calculating according to the principles described above, we may conclude that a pressure of 530 atmospheres would have caused the action to take place at double the rate, and that therefore the chemical action is equivalent to the expenditure of that amount of

mechanical force, being thus generated by it. Arguing then in a manner similar to that already described, but modified to suit the different conditions, if the contraction is equal to 8 per cent. of the bulk of the calcite, there must be a loss of mechanical force capable of raising 28 times the weight of the calcite altered to the height of 1 metre, in the time required for the chemical change; which amount of mechanical energy, as it were, becomes latent, and is transformed into chemical action, and would again exhibit itself as a mechanical force if, by any means, the chemical affinities could be inverted and everything restored to its original state.

In a like manner, other experiments indicate that in some cases pressure causes a slower, and in others a quicker chemical action, whilst in others it has scarcely any influence whatever; and though, for reasons already explained, I say it with some hesitation, yet, bearing in mind what is already known respecting the action of pressure on hydrate of chlorine, hydrated hydrosulphuric acid, and other substances described by the various authors referred to in the notes, I think the facts I have described make it very probable that further research will show that pressure weakens or strengthens chemical affinity according as it acts against or in favour of the change in volume; as if chemical action were directly convertible into mechanical force, or mechanical force into chemical action, in definite equivalents, according to well-defined general laws, without its being necessary that they should be connected by means of heat or electricity. On the present occasion I shall not attempt to consider the various geological and mineralogical facts which appear to me to admit of the application of the principles I have described, for many of them are peculiarities in structure of which neither myself nor any one else has ever given a description, and would therefore demand a preliminary notice. However, I may say that it appears to me that a number of facts connected with metamorphic rocks and the phenomena of slaty cleavage, which, to me at all events, have hitherto been inexplicable, are readily explained if mechanical force be directly correlated to chemical action, and if in some cases the direction in which crystals are formed is more or less related to pressure, in some such way as there is a connexion between their structure and magnetic force, as shown by the experiments of Plücker, Faraday, Tyndall, and many other observers. We may also, I think, explain the origin of the impressions on the limestone pebbles in the "Nagelfluë" in Switzerland, about which so much has been written in Germany and France, without a satisfactory reason having been discovered; and the same explanation accounts for the mutual penetration of the fragments of which some limestones are formed, and for the banded structure of some which possess slaty cleavage. The curious teeth-like projections with which one bed of limestone sometimes enters into another, also to a certain extent indicate a chemical action depending on mechanical force; and probably the same may be said of some of the peculiarities of slickensides and mineral veins. It is also possible that a pressure

of several hundred atmospheres may facilitate some of the chemical changes involved in the transformation of water and carbonic acid into the organic compounds met with in animals and plants of low organization found at great depths in the ocean, and thus to a certain extent compensate for diminished light. I, however, most willingly admit that very much remains to be learnt before we can say to what extent the principles I have described are applicable; and yet, at the same time, cannot but think that henceforth they must be taken into account in many departments of chemical and physical geology, and will readily explain a number of facts which otherwise would be very obscure.

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GEOLOGICAL SOCIETY.

[Continued from p. 74.]

December 16, 1863.—R. A. C. Godwin-Austen, Esq., Vice-President, in the Chair.

The following communications were read :—

1. "On the Pebble-bed of Budleigh Salterton." By W. Vicary, Esq., F.G.S. With notes on the Fossils, by J. W. Salter, Esq., F.G.S.

The south coast of Devonshire from Petit Tor, near Babbacombe Bay, to a little beyond Sidmouth, exhibits cliffs of New Red Sandstone, one of the beds of which, near Budleigh Salterton, is composed of pebbles of all sizes and of a flattened oval form; this bed attains a maximum thickness of about 100 feet, and some of the pebbles composing it were found by Mr. Vicary to contain peculiar fossils.

Mr. Vicary gave a description of the physical features of the area over which the pebble-bed extends, and entered into the stratigraphical details of this and the associated strata, referring to Mr. Salter's Note for information upon the affinities of the fossils.

In his Note, Mr. Salter observed that, on comparing the fossils of the Budleigh-Salterton pebbles with those from the Caen sandstone in the Society's Museum, he found that all the species contained in the latter collection were also represented in the former. The general aspect of the fossils was stated to be quite unlike that exhibited by English Lower Silurian collections; and Mr. Salter therefore suggested that the exact equivalent of the Caen sandstone does not exist in England. This difference in the two faunas appeared to him to favour the theory of the former existence of a barrier between the middle and northern European regions during the Silurian period.

2. "Experimental Researches on the Granites of Ireland.—Part IV. On the Granites and Syenites of Donegal, with some remarks on those of Scotland and Sweden." By the Rev. Samuel Haughton, M.D., F.R.S.

The author discussed in detail the mineralogical composition of



each of the fifteen Donegal granites, and described the method usually employed by him in solving lithologico-chemical problems, coming to the conclusion that nearly half of these granites are not composed altogether of the four minerals (Quartz, Orthoclase, Oligoclase, and Black Mica) which are found in them in distinct crystals, and that the remaining varieties, even if they be composed of these minerals, must have a paste composed of the same minerals, but with a slightly different composition. Prof. Haughton then discussed the composition of the syenites of Donegal, and instituted a comparison between the granites of that district and those of Scotland and Sweden, remarking that those of the last-named region have the same stratified structure as the granites of Donegal.

3. "On the recent Earthquake at Manila." By J. W. Farren, Esq. Communicated by the Foreign Office.

In two letters to Earl Russell the author described the damage done by this earthquake, observing that 289 persons were killed, and a large number more or less injured.

4. "Extracts from letters relating to the further discovery of Fossil Teeth and Bones of Reptiles in Central India." By the late Rev. S. Hislop. Communicated by Prof. T. Rupert Jones, F.G.S.

The remains alluded to consist of (1) a series of Reptilian bones, some bearing teeth, mostly Labyrinthodont, and some probably Dicynodont, from the (Triassic?) red clay of Maledi, in which teeth of *Ceratodus* occur; and (2) several teeth similar to one from the Eocene clays of Takli, near Nagpore, and another like a conical tooth from the Eocene beds (with *Physa Prinsepîi*) of Physura, from the same neighbourhood as that in which the set No. 1 was found.

At Phisdura (Tertiary), large Reptilian bones (including a femur 1 foot across at the condyles, and a vertebral centrum 7 inches across) have been found associated with large coprolites, *Physa Prinsepîi*, and *Paludina Deccanensis*.

Mr. Hislop stated his belief that the Mangali beds, the Korhadi shales, and the red clay of Maledi, should be placed above the plant-bearing beds of Nagpore instead of below them, as heretofore supposed.

January 6, 1864.—Professor A. C. Ramsay, President,  
in the Chair.

The following communications were read:—

1. "On the recent Geological Changes in Somerset, and their date relatively to the Existence of Man and of certain of the Extinct Mammalia." By G. S. Poole, Esq.

In describing the general physical features of the district treated of, which lies between Clevedon and Taunton, the author noticed especially the embankments which protect the land from periodical inundations, stating his belief that they were constructed by the Romans; he also described the "turbaries" or peat-moors, endeavouring to explain their mode of formation, and noticing their

relation to the alluvium and the sand-banks of the district. Mr. Peole then endeavoured to prove that the area under consideration had been subject to considerable changes of level in comparatively recent geological times, and that Man existed in the district prior, and some of the extinct Mammalia subsequently, to the last of such changes, asserting, in support of the last conclusion, that the remains of *Elephas primigenius*, *Rhinoceros tichorhinus*, &c., had been found in a stratum above that containing the bones of Man and pieces of pottery; and he concluded by examining the evidence of the extent and date of the last subsidence.

2. "On the Structure of the Red Crag in Suffolk and Essex." By Searles V. Wood, Jun., Esq.

By reference to a tabulated description of about fifty sections taken from various parts of the Red-crag area, the author showed that the deposit is structurally divisible into five stages, of which the 1st, 2nd, 3rd, and 4th (counting upwards) were not deposited under water; but from their being regularly laminated, at angles varying between  $25^{\circ}$  and  $35^{\circ}$ , and possessing (with the exception of the 2nd) an unvarying direction in every stage, he regards them as the result of a process of "beaching up," by which was formed a reef extending from the river Alde on the north, to the southern extremity of the deposit in Essex. Of these four stages, the 4th is the most constant and important, the 1st, 2nd, and 3rd being frequently either concealed by, or destroyed during the formation of, the succeeding stages. At Walton-on-the-Naze alone do any of the four lower stages contain evidence of being a subaqueous deposit; there the 1st stage is so, but it is covered by two reef stages, and these again by the 5th stage.

The 5th stage is invariably horizontal, and contains evidence of having been formed under water. This stage is developed in such a way as to show that it was formed in channels eroded in the older reef, and it is at its base that the coprolite workings occur. This stage also passes up at Chillesford into the sands and gravels termed by the author the Lower Drift, which underlie the boulder clay; at other places a line of erosion exists between the 5th stage and the drift-sands.

## XXVIII. *Intelligence and Miscellaneous Articles.*

SOME REMARKS ON THE TELESCOPIC APPEARANCE OF THE EXTERIOR ENVELOPE OF THE SUN, AND OF ITS SPOTS. BY THE REV. W. R. DAWES.

THE recent increase in the number of powerful telescopes armed with the means of observing the sun without danger to the eye or to the dark glasses, has caused a corresponding increase in the number of observers of solar phenomena. These, when carefully scrutinized with large apertures and high powers under suit-

able atmospheric circumstances, are so wonderfully different in their appearance from those presented by the diminished apertures formerly and necessarily in use, that it would not be very surprising if some observers, unaware of what had previously been seen and described, should imagine that the phenomena revealed by their newly acquired and powerful telescopes were really *new discoveries*. And this is what, there is good reason to believe, has in some instances actually occurred. Such a mistake may also be more likely to be made when a *new name* has been applied by some recent observer to an appearance long familiar to others, though previously unnamed. A name, no doubt, has the advantage of affording a convenient handle whereby to grasp the thing described; but, unless it is very correct and appropriate, it conveys an erroneous impression of the appearance to which it is applied, and may become far more injurious than advantageous. It seems to me, therefore, to be desirable to direct attention to what has been long ago observed and described, and also to put on record some results of a pretty constant scrutiny of solar phenomena with powerful and excellent telescopes during the last twelve or fifteen years.

The mottled appearance of the solar surface requires no very large amount of optical power to render it visible. I have often observed it with a good refractor of only  $2\frac{1}{2}$  inches aperture and a power of 60. Examined with a large aperture, such as 6 or 8 inches, it becomes evident that the surface is principally made up of luminous masses imperfectly separated from each other by rows of minute dark dots,—the intervals between these dots being extremely small, and occupied by a substance decidedly less luminous than the general surface. This gives the impression of a division between the luminous masses, especially with a comparatively low power, which, however, when best seen with high powers, is found to be never complete. The masses thus incompletely separated are of almost every variety of irregular form—the rarest of all, perhaps, being that which is conveyed to my mind by Mr. Nasmyth's appellation of "*willow leaves*," viz. *long, narrow, and pointed*. Indeed the only situation in which I have usually noticed them to assume anything like such a shape, is in the immediate vicinity of considerable spots, on their *penumbrae*, and frequently projecting beyond it irregularly for a small distance on to the *umbra*,—an appearance with respect to which, in my "Description of a new Solar Eyepiece," which I read before the Society in April 1852 (see 'Memoirs,' vol. xxi. p. 161), I employed the following expressions:—"The interior edge of the penumbra frequently appears extremely jagged—the bright ridges on its surface, which are directed nearly towards the centre of the spot, being seen projected to irregular distances on to the cloudy stratum [or *umbra*], and looking much like a piece of coarse thatching with straw, the edge of which has been left untrimmed." After nearly twelve years of careful observations of the same phenomena, I do not think that I could improve upon this description.

Of the general appearance of the sun's surface, I have met with



no more accurate description than is contained in Sir John Herschel's 'Outlines of Astronomy,' art. 387, where he says, "The part of the sun's disc not occupied by spots is far from uniformly bright. Its ground is finely mottled with an appearance of minute dark dots or *pores*, which, when attentively watched, are found to be in a constant state of change. There is nothing which represents so faithfully this appearance as the slow subsidence of some flocculent chemical precipitates in a transparent fluid, when viewed perpendicularly from above." The only part of this admirable description which I should be at all disposed to modify is that relating to the "constant state of change" in the *pores*. The plan I have uniformly adopted in the minute scrutiny of the solar surface has been to employ very small perforations in the diaphragm-plate of my solar eyepiece (viz. from 20" to 60" in diameter), using the highest magnifiers which circumstances would permit. In this way I have frequently kept the same luminous masses and the intervening pores in view for an hour or two together—and have rarely observed any decided change in either, even when well seen with powers from 400 to above 600. Yet very slight atmospheric tremors suffice to give an appearance of almost perpetual variation; and unless the view is confined within the narrow limits of a very small field, the eye is apt to become confused in its judgment of the relative positions of objects so minute and delicate.

A striking exception, however, to this comparative quietude is found in the immediate vicinity of spots which are either rapidly enlarging or closing. It is under these circumstances especially that the luminous masses are found to become more elongated. This is also more remarkably the case when they are *preparing for a rush across a chasm*, and thus forming those luminous bridges which so often intersect considerable spots. The point from which such a rush is about to be made is often indicated by a greater crowding together towards that place, and a general inclination of the longer axis of each of the elongated masses in that direction; which might, I imagine, be well exemplified by such chemical precipitates as Sir John Herschel alludes to, if they were about to flow through a narrow spout or opening in the vessel containing them. I have had my attention thus specially directed to such a point at the edge of a spot, and, having placed it in a very small field of view, have watched with great interest the formation of the first portion of the bridge. In this process the luminous masses have the appearance of bits of straw, all lying nearly in the same direction, though sometimes rather obliquely to the line of the bridge—the sides of the bridge having a jagged appearance, arising from the unequal length of the pieces which compose it. It is a remarkable fact that these bridges are always formed by the bright streaks of the outer stratum, such as are seen projected on the penumbra, without any admixture of the less luminous second or penumbral stratum. Such, at least, is the conclusion I have arrived at after frequent and very careful scrutiny. And so intense is the light of such a line, however narrow, that when

projected on to the umbra of a large spot, its superior brightness usually prevents the cloudy stratum from being discerned at all, even when the umbral portion only is included in the small field of view.

With respect to the highly interesting subject of the origin or cause of the solar spots, I wish to direct special attention to the *black nucleus* which in most of the large and symmetrical spots is found to occupy some part of the *umbra*. I cannot but regret that, though nearly twelve years have elapsed since I announced the frequent existence of such a black spot, and that it would be highly desirable to restrict to it the term *nucleus*, yet I believe the distinction has never been made by any other observer—the *umbra* having been universally, so far as I know, designated as the *nucleus*, and the existence of the true black nucleus within it either forgotten or ignored. My earnest desire that the distinction should be recognized and established does not arise merely from the conviction that accuracy is preferable to inaccuracy, and that where a decided difference really exists it is desirable to express it by a difference of terms, though I should imagine that such a ground would generally be deemed sufficient of itself; a still more cogent reason is found in the fact, that the whole series of my observations for twelve years past has led me to the conclusion that generally, if not universally, the existence or absence of the black nucleus is sufficient to determine, or at least to throw much light upon, the *origin* of the spot, and that the origin of those in which the nucleus exists is widely different from the origin of those from which it is absent. To do justice, however, to this view of the subject would unduly lengthen the present paper; and I therefore hope to be able, in a future communication, more fully to lay before the Society the conclusions at which I have arrived.—*From the Monthly Notices of the Royal Astronomical Society*, December 11, 1863.

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#### ON THE RAYS OF THE ULTRA-VIOLET SOLAR SPECTRUM.

NOTE BY M. MASCART.

I have the honour to lay before the Academy a drawing of the ultra-violet solar spectrum with most of the rays which it contains. The method used to obtain it is the exact copy of that which serves for the observation of the luminous spectrum. A collimator goniometer was used, the two lenses of which are of quartz cut perpendicularly to the optic axis, so that the rays traverse it only slightly inclined in reference to this axis. The refracting prism is also of quartz, cut parallel to the axis; and I generally observed the extraordinary spectrum, which is the most divergent. Such a system of lenses is not achromatic; but this does not prevent a pure spectrum being obtained, and has no other effect than that of causing a considerable change of position in passing from the less to the more refrangible rays. In the mounting of the eyepiece of the telescope is a cross wire, and at the inner extremity of the eyepiece is a photographic

plate the face of which is exactly behind the eyepiece, so as to take the impression of the phenomenon which is produced in its plane.

By this arrangement the chemical region which is to be examined can be easily put in position, although it is invisible. It is merely necessary to produce in the plane of the eyepiece a sharp image of the extreme limit of the luminous spectrum, the ray H for instance; the eyepiece is then removed a little so as to bring the point towards the more refrangible parts, and an experiment is made by replacing the ordinary eyepiece by the photographic eyepiece. The examination of the proof soon shows in what direction the cross wire is to be moved. The want of achromatism of the lenses gives precision only within very narrow limits; and as the energy of the action differs in different parts, the time of exposure ought to differ also. Hence the experiments must be multiplied, and no less than eight trials are necessary for producing the chemical spectrum quite complete.

It is readily conceivable that great delicacy of detail can be obtained; for the rays concentrated on a very small surface always retain a sufficiently energetic action, however large the slit. I used collodion sufficiently sensitive to give an ordinary photograph in five or six seconds; and the time of exposure never exceeded a minute and a half. The proofs taken can be placed in a solar microscope and enlarged positives obtained, but the results are imperfect from the limited space which is in position; it is better to examine them in the microscope, and measure the distances of the rays with a micrometric screw placed on the stage, and draw them carefully: I have obtained my drawing in this manner. The distances are not quite proportionate to the deviations, owing to the slight variations of enlargement; I endeavoured especially to obtain the general aspect, the form of each group, and the relative intensity of the rays.

Many physicists, especially MM. Becquerel, Stokes, and Esselbach, have already worked at this question, and designated by letters the groups of the principal rays. Their nomenclature does not always agree; the drawings are sometimes so imperfect as to make it difficult to recognize them; and the names have been given sometimes to the bright and sometimes to the dark spaces of the spectrum. I have taken as guide the plate published by Müller in his *Lehrbuch der Physik*, applying each letter to the most remarkable obscure ray of the group it served to designate.

To illustrate the precision attainable, it may be remarked that the luminous spectrum described by Fraunhofer comprises 320 rays from A to H, and that in an almost equal angular space from H to I I could indicate more than 280: the results are thus comparable to those obtained with light. The dispersion can be increased by multiplying the prisms, as Kirchhoff has done in examining the luminous chemical rays of coloured flame and compared them with the dark rays of the solar spectrum. I propose to resume this subject when circumstances permit.—*Comptes Rendus*, November 9, 1863.



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[FOURTH SERIES.]

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MARCH 1864.

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XXIX. *On the Optical Properties of the Metals.*  
By G. QUINCKE\*.

WHILST physicists have for some time back occupied themselves with the properties of the light reflected from metals, they have paid very little attention to light passing through metals—a circumstance partly to be ascribed to the technical difficulties of the preparation and manipulation of thin transparent metallic plates. The little that one knows about the light transmitted through metals, as, for instance, the results of Faraday's† recent researches, refers almost entirely to the intensity and colour of the light. The latter shows itself to be so very inconstant for the same metal, that one would be disposed to seek the explanation of the irregularity in the presence of holes in the metallic plate, if Faraday had not demonstrated this property of a thin metallic film, viz. that when placed obliquely between two crossed Nicol's prisms, it illuminates the field and acts "just like a glass plate."

This property of thin transparent plates of metal was, so far as the author knows, first observed by Warren De la Rue‡ with regard to gold-leaf, and afterwards by Faraday in thin transparent plates of platinum, palladium, rhodium, silver, copper, tin, lead, iron, zinc, and aluminium§. From a remark of Fara-

\* Translated by Professor Wanklyn from Poggendorff's *Annalen*, vol. cxix. part. 3 (1863, No. 7).

† Phil. Trans. 1857, p. 145. Experimental Researches in Chemistry and Physics, vol. iv. p. 391.

‡ Faraday, 'Experimental Researches,' vol. iv. p. 401.

§ Ibid. p. 441.

day's\*, however, that he and Warren De la Rue found the colour of the light transmitted by gold to be altered by the employment of polarized light and a set of gypsum plates, it follows that the metallic plates must act *somewhat* differently from glass plates. The author has arrived at the following results in the course of his research on the properties of the light transmitted through metals.

When a ray of plane-polarized light is reflected from a metallic surface, the two components of the ray polarized parallel and perpendicular to the plane of incidence undergo a different alteration in phase by reflexion. Jamin† has shown that the difference of phase of the components increases from 0 to  $\pi$  (corresponding to a difference in the path of the components varying from zero to the length of half an undulation  $\frac{\lambda}{2}$ ) if the angle of incidence increases from  $0^\circ$  to  $90^\circ$ , *i. e.* from a perpendicular to a grazing incidence. The investigation, carried out with the aid of Babinet's compensator, showed that the component polarized parallel to the plane of incidence was always accelerated in relation to the component polarized perpendicular to the plane of incidence. If the components of the incident light polarized parallel and perpendicular to the plane of incidence were of equal intensity (that is, if the incident ray were polarized under an azimuth  $\alpha = 45^\circ$ ), the intensity of the component polarized parallel to the plane of incidence always preponderated in the reflected light.

If a ray of light, polarized in the azimuth  $\alpha = 45^\circ$ , be passed through transparent leaves of gold, silver, or platinum, and if the difference of phase of the components be determined by means of a compensator of Babinet's construction, it will be found that (as in reflected light) the component polarized parallel to the plane of incidence will predominate over the component polarized perpendicular to the plane of incidence, and that the difference of phase will be 0 when the incidence is perpendicular, and will increase with an increasing angle of incidence.

The difference of phase of the components is always less in transmitted than in reflected light under the same angle of incidence; and the author has never been able to observe a greater difference of phase than  $\frac{\pi}{2}$ , corresponding to a difference of path of  $\frac{\lambda}{4}$ . And in these experiments it makes a difference whether

\* Experimental Researches, vol. iv. p. 394. Employing polarized light and an arrangement of sulphate of lime plates, it was found that other rays than the green could be transmitted by the gold-leaf.

† *Ann. de Chim. et de Phys.* (3) vol. xix. (1847) p. 296; (3) vol. xxix. (1850) p. 282.

the transparent metallic leaf is bounded on both sides by air, or on one side by air and on the other by a highly refracting medium such as glass.

When the components of an incident ray, polarized parallel and perpendicular to the plane of incidence, have an equal intensity, then in the *transmitted* light the intensity of the component polarized perpendicular to the plane of incidence preponderates.

As yet it is only in the case of gold that the author has succeeded in obtaining leaves which were bounded on both sides by air, and at the same time sufficiently uniform and even. For this purpose gold-leaf of a convenient shape was placed upon distilled water and allowed to straighten itself out. It was then lifted off by means of a small frame of brass which had been previously burnt black, and of which the inside measure was 4 millims. in breadth and 28 millims. in length. After the evaporation of the water there thus remained in the frame a transparent gold plate which could be fastened by means of the frame to the axis of a goniometer and examined. Such a plate is never quite flat, and consequently there is always an error in the measurement of the angle of incidence, and this error may amount to several degrees.

The goniometer had a perfectly similar disposition to that which Jamin\* employed in his researches. Two brass tubes blackened internally could be turned on a graduated circle about the axis of the goniometer, to which they were directed perpendicularly, and carried at their extremities two circles with Nicol's prisms, in order to allow of the determination of the plane of polarization of the incident and emergent rays. The rays reflected from or passing through metal were received by a Babinet's compensator previously to their entry into the Nicol's prism used as the analyzer, in order to neutralize the difference of phase of the components polarized parallel and perpendicular to the plane of incidence. The rays emerging from the compensator were then polarized in a straight line, and the azimuth  $\beta$  of their plane of polarization could be measured by means of the analyzing Nicol's prism, by turning it until a black band again appeared between the parallel lines of the compensator; at this spot the light emerging from the compensator was fully extinguished.

The brass tube which carried the polarizing Nicol's prisms was provided with two diaphragms with cross lines, and the entire apparatus was so placed that the shadows of both cross lines, received on a plate of ground glass, covered one another when a heliostat projected sunlight upon the Nicol's prism and then

\* *Ann. de Chim. et de Phys.* vol. xix. p. 370 et seq.; vol. xxxi. p. 170.



upon the cross lines. Thereby the direction of the incident light was fixed. The metal plate stood perpendicular to the same, and parallel to the axis of the goniometer, when the rays reflected from it were thrown back upon the cross lines next to it.

If we call  $\alpha$  the azimuth of the incident light, then the ratio  $k$  of the amplitudes S and P of the components of the light polarized perpendicular and parallel to the plane of incidence, when these have equal intensity in the incident light, is given by the equation

$$k = \frac{S}{P} = \frac{\tan \beta}{\tan \alpha}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Putting  $a$  for the number of revolutions of the compensator which are required to give to the rays leaving the compensator a difference of path equal to a quarter of an undulation  $\frac{\lambda}{4}$ ,  $\rho$  for the number of the revolutions read on the compensator necessary to eliminate the difference of path  $\delta$  of the components of the light which is either reflected from metal or transmitted through metal, then is  $\delta$ , expressed in fractions of a quarter of an undulation,

$$= \frac{\rho}{a} \cdot \frac{\lambda}{4}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The following Table gives under I the angle of incidence, under  $\rho_0$  and  $\rho_{180}$  the observed revolutions of the compensator according as one side or the other of the gold-leaf is presented to the source of light; under  $\beta_0$  and  $\beta_{180}$  are given the corresponding values of the restored polarization; under  $\rho$  and  $\beta$  the means of these values. Since the incident light was polarized under the azimuth  $\alpha = 45^\circ$ , it follows that the tangent  $\beta$  gives directly the ratio  $k$  of the amplitudes of the components polarized perpendicularly and parallel to the plane of incidence. For comparison, the values  $\rho$  and  $\beta$  are given for light which had been reflected from a portion of the same gold-leaf. This last piece of gold-leaf had been placed on distilled water, taken up with a plate of glass, and then dried on the same plate of glass. Between the eye and analyzing Nicol's prism a piece of gold-leaf was inserted in the experiments with reflected light, in order to have light of the same colour as that obtained in the experiments on transmitted light.

TABLE I.

$\alpha = 45^\circ.$ Transmitted light.								$a = 6^{\circ} 9' 44''.$ Reflected light.		
I.	$\rho_0.$	$\rho_{180}.$	$\rho.$	$\delta.$	$\beta_0.$	$\beta_{180}.$	$\beta.$	$\rho.$	$\delta.$	$\beta.$
0	r	r	r	0	46° 32'	44° 30'	45° 31'			°
10	0.038	.....	0.038	0.0054	44 34	45 48	45 11			
20	0.222	0.236	0.229	0.0330	48 35	46 6	47 20	0.656	0.0945	45 50
30	0.548	0.664	0.606	0.0873	49 31	49 52	49 41	1.438	0.2070	46 45
40	1.444	1.218	1.331	0.1917	54 15	54 24	54 19	2.124	0.3059	43 15
50	2.382	2.400	2.391	0.3443	59 40	58 57	59 18	3.140	0.4522	42 37
60	3.230	4.144	3.687	0.5309	63 17	65 22	64 19	5.576	0.8024	40 23
70	5.562	5.324	5.442	0.7838	68 37	67 36	68 6	6.800	0.9792	41 11
75	6.840	6.783	6.811	0.9808	70 27	66 39	68 33	8.124	1.1700	43 2
80	.....	.....	.....	.....	.....	.....	.....	10.226	1.4730	42 21
85	.....	.....	.....	.....	.....	.....	.....	11.622	1.6740	43 27

That in perpendicular incidence  $\beta$  was not exactly equal to  $45^\circ$ , arose from the circumstance that the gold plate was not quite even, but slightly cylindrical, inasmuch as it hung in the frame like a cloth suspended by the corners. Owing to this the angle of incidence was therefore not  $0^\circ$ , but greater, and indeed situated in a plane of incidence which, parallel to the axis of the goniometer, stood at right angles to the plane of incidence for which the angles of incidence are given under I.

With other gold-leaves similar values were obtained. Warren De la Rue and Faraday\* have found the thickness of a gold-leaf to be about 0.000091 millim.

The values of the difference of phase, or accordingly of  $\rho$ , turn out much less when the gold has one of its surfaces in contact with glass and the other with the air. The following Table contains the observations made upon a film of gold spread upon a plane-parallel glass according to the method of Liebig†. The gold film was thinner than gold-leaf, and transparent, and of a green-blue colour. The angle of polarization of the glass was  $57^\circ 35'$ , corresponding to an index of refraction of 1.575.

The values of  $\rho$  are given under  $\rho_0$  and  $\rho_{180}$ , in the one case the glass, and in the other the metal being turned towards the heliostat; under  $\beta_0$  and  $\beta_{180}$  are given the corresponding values of  $\beta$ ; under  $\beta_g$  the azimuth of the light is given when the light has passed only through the glass plate and not through metal.

\* Experimental Researches, vol. iv. p. 393.

† Liebig's *Annalen*, April 1856, p. 132. Dingler's *Journal*, 1856, vol. cxi. p. 199 *et seq.*

Before the eye was placed a red glass that specially transmitted the rays of the spectrum between the Fraunhofer's lines B and D.

TABLE II.

$\alpha = 45^\circ$ . Transmitted light.									Reflected light.		
I.	$\rho_0$ .	$\rho_{180}$ .	$\rho$ .	$\delta$ .	$\beta_0$ .	$\beta_{180}$ .	$\beta$ .	$\beta_g$ .	$\rho$ .	$\delta$ .	$\beta$ .
$^\circ$	$''$	$''$	$''$		$^\circ$	$^\circ$	$^\circ$	$^\circ$			$^\circ$
0	0	0	0	0	45 20	46 44	46 2	45 20			
10	0.054	0.058	0.056	0.0074	46 5	45 20	45 42	46 5			
20	0.224	0.176	0.200	0.0263	46 20	46 47	46 33	46 7	0.430	0.0566	45 32
30	0.466	0.496	0.481	0.0632	46 53	47 53	47 23	46 33	1.060	0.1394	40 58
40	0.858	0.858	0.858	0.1128	49 3	49 57	49 30	47 30	2.102	0.2765	39 9
50	1.466	1.514	1.490	0.1960	52 12	51 15	51 43	48 8	2.556	0.4677	34 55
60	2.044	1.712	1.878	0.2470	55 18	54 36	54 57	50 12	5.700	0.7498	31 51
70	2.912	2.484	2.698	0.3550	62 20	59 25	60 52	56 18	8.860	1.086	29 45
80	3.784	3.146	3.465	0.4557	66 55	64 31	65 43	60 47	11.938	1.570	33 28
85	3.968	.....	3.968	0.5219	67 25	.....	67 25	63 39	13.498	1.775	38 23

From these numbers it appears that the difference of phase and the azimuth are the same, whether the light goes first through metal and then through glass, or first through glass and then through metal. The difference of phase is, however, much smaller than when the metal has both its surfaces in contact with the air. The reason of this diversity is partly that, owing to refraction at the surface of the glass, the angle of incidence under which the rays going through the glass meet the metallic plate is smaller than the corresponding value under I, which expresses the angle of incidence under which the rays in air fall upon the glass plate bearing the metallic plate.

The following Table contains the observations upon a film of silver of a blue-grey colour, and of a thickness determined according to the method of Fizeau\* to be 0.000076 millim. Fizeau's method depends upon the colours which are produced by the action of a minute portion of iodine on the silver film. The silver film was obtained on parallel glass by the method of Petitjean†. From the angle of polarization  $56^\circ 58'$ , it follows that the index of refraction of the glass plate was  $n = 1.538$ .

A red glass was placed before the eye. The letters have the same meaning as in the former Tables.

\* *Comptes Rendus*, vol. lii. 1. 1861, p. 274.

† *Pogg. Ann.* vol. ci. p. 313.



TABLE III.

$\alpha = 45^\circ$ . $a = 7.603$ . Transmitted light.										Reflected light.		
I.	$\rho_0$ .	$\rho_{130}$ .	$\rho$ .	$\delta$ .	$\beta_0$ .	$\beta_{130}$ .	$\beta$ .	$\beta_g$ .		$\rho$ .	$\delta$ .	$\beta$ .
0	r	r	r	.....	45° 54'	45° 42'	45° 48'	45° 10'				°
10	0	.....	.....	.....	46	4 46	31 46	18 46	16			
20	0.098	0.156	0.127	0.0167	46 36	47 26	47 1 46	39		0.088	0.0116	44 18
30	0.386	0.508	0.447	0.0588	48 47	48 47	48 47	46 50		0.756	0.0994	41 54
40	0.770	0.788	0.779	0.1024	50 50	53 50	26 47	14		1.764	0.2320	40 29
50	1.296	1.320	1.308	0.1731	55 65	4 34	34 48	48		2.874	0.3780	36 9
60	1.636	1.984	1.810	0.2381	57 49	57 36	57 42	49 59		4.728	0.6218	33 1
70	2.324	2.350	2.337	0.3074	62 44	61 56	54 53	57		7.814	1.028	31 17
80	3.128	3.108	3.118	0.4101	68 39	67 21	68 59	28		11.146	1.467	33 23
85	3.386	3.960	3.673	0.4832	71 18	72 35	71 56	61 32		13.166	1.732	36 34

Other transparent plates of silver gave similar results, no matter whether their colour in transmitted light was yellow, grey, blue, or violet; and no matter whether prepared by Petitjean's method of reduction with tartaric acid, or by Liebig's method of reduction with sugar of milk. The difference of phase is for the same angle of incidence nearly the same, and seems to be independent of the thickness of the film. The azimuth  $\beta$ , on the contrary, depends upon the thickness and the want of transparency of the silver, being for the same angle of incidence the smaller in proportion as the metallic film is thinner and more transparent. On account of the many modifications and conditions in which silver, like other metals, occurs, and which will be again alluded to further on, the author is unable to give any more determinate results, although he has made many measurements.

The thickness of the plates employed varied from 0.000062 millim. to 0.0001827 millim. Thicker plates allowed too little light to pass; so that it was no longer possible to observe with certainty, especially at the larger angles of incidence, at which indeed the thickness of the silver plate to be traversed by the light was increased.

Finally the observations made on a platinum film will find a place. This platinum film was obtained by heating the solution of a platinum salt in an ethereal oil (for which the author is indebted to Professor Böttger) to  $600^\circ$  C. or higher, the solution having been first spread on a piece of plate glass. The platinum was thus obtained in the form of a reflecting film, which had a grey colour and was transparent. The metallic surface was turned towards the heliostat. The reflected light was made to pass through a red glass before reaching the eye, on which

account the values of  $a$  for transmitted light differ slightly from those for reflected light.

TABLE IV.

$\alpha = 45^\circ.$ $a = 7^r.128.$ Transmitted light.					$\alpha = 45^\circ.$ $a = 7^r.603.$ Reflected light.		
I.	$\rho.$	$\delta.$	$\beta_0.$	$\beta_g.$	$\rho.$	$\delta.$	$\beta.$
0	$0^r$	0	$45^{\circ} 35'$	$45^{\circ} 35'$	0	0	0
20	0.522	0.0732	46 49	45 27	0.374	0.0492	44 41
30	0.904	0.1269	51 20	46 15	0.760	0.1000	43 46
40	1.178	0.1653	56 27	45 59	1.094	0.1439	42 49
50	1.484	0.2082	63 10	49 25	1.774	0.2333	39 51
60	2.086	0.2927	69 59	50 50	2.892	0.3804	35 20
70	2.386	0.3321	77 45	51 40	4.842	0.6368	32 5
75	2.766	0.3880	80 24	54 50	6.444	0.8476	28 17
80	3.728	0.5230	83 2	55 10	8.782	1.155	27 29

Observations made upon other transparent plates of platinum furnished similar results. Here again, just as for silver, the difference of phase and the azimuth  $\beta$  (under the same incidence) were found to be the greater the less transparent the films.

But it is impossible to form a just idea of the thickness of the films from their degree of transparency; for the same metal presents various degrees of transparency, as has been shown by Faraday. In the optical properties of the metals, and in the influence which they exercise upon reflected or transmitted light, there was observed the widest differences—and, indeed, not alone as regards silver, which one might suppose to undergo oxidation, but also with regard to gold and platinum, of which this supposition is inadmissible.

The optical constants of the metals may be calculated, according to Cauchy's theory, from the principal angle of incidence  $A$ , for which the difference of velocity of the components, polarized parallel and perpendicular to the plane of incidence, becomes  $\frac{\lambda}{4}$ , and from the azimuth  $H$  which belongs to it, and which represents the restored polarization when the incident light was polarized in the azimuth  $\alpha = 45^\circ$ .

Cauchy has given only the data and the results of his calculation. A more detailed development is to be found in the papers of Beer\* and Eisenlohr†, the latter of whom gives formulæ which enable us to calculate with accuracy the refraction and extinction-coefficients at perpendicular incidence.

\* Pogg. Ann. vol. xcii. p. 402.

† Ibid. vol. civ. p. 374.

From the experiments of Jamin, Eisenlohr has deduced the values of the optical constants which he denotes by  $\epsilon$  and  $\mathfrak{S}$  for a series of metals. The index of refraction  $n$ , and the extinction-coefficient  $g$  for light falling perpendicularly on the metallic plates, are related to these constants so that

$$n = \mathfrak{S} \cdot \cos \epsilon, \quad g = \mathfrak{S} \cdot \sin \epsilon. \quad . \quad . \quad . \quad . \quad (3)$$

For other angles of incidence  $I^*$ , the index of refraction  $\nu$ , and the extinction-coefficient  $\gamma$ , have the values

$$\left. \begin{aligned} \nu^2 &= n^2 + \sin^2 I, \\ \gamma^2 &= g^2 + \sin^2 I. \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (4)$$

The index of refraction, therefore, in the case of the metals is dependent upon the angle of incidence.

The values of  $n$  and  $g$  which Beer†, by means of an approximation, deduces from the observations of Jamin, differ from those which are furnished by Eisenlohr's calculation. By both methods, however, the refraction index  $n$  of silver is given less than unity; in other words, light ought to travel through silver with greater velocity than through a vacuum.

Light penetrates, as the above researches show, to an appreciable depth into metal. It must also, according to the view entertained by the author, be reflected back from the interior; for the great difference of phase of the components of reflected light seems to be only explicable on the supposition that the reflected ray has to pass twice through the boundary between the metal and the medium lying adjacent to it. The author's measurements show that the light penetrates into the metal to a depth which is as great as, or very little less than the length of an undulation; and Dove‡ remarked, a long time ago, how the so-called metallic lustre was produced by a conjoint action of exteriorly reflected and interiorly reflected or dispersed light.

Since the theory, apart from the assumptions which it makes, takes no account of this penetration of light into metal, the author has endeavoured to determine directly the velocity of light through metals. He has obtained, in the course of this investigation, the remarkable result that light travels faster through gold and silver than through a vacuum.

For these experiments an interference-apparatus was used which had previously been employed in other optical investigations§. A heliostat reflected sunlight in a horizontal direction upon a vertical slit which was situated in the focus of an achro-

\* Beer, Pogg. *Ann.* vol. xcii. p. 412.

† Ibid. p. 417.

‡ *Monatsber. d. Berl. Akad.* 1851, p. 262.

§ Ibid. 1862, pp. 718, 719; and Pogg. *Ann.* vol. cxviii. p. 449.



matic lens. The rays emerged nearly parallel from the lens, and then encountered a system composed of two plane-parallel plates of glass of exactly equal thickness (8.66 millims.), and placed 200 millims. apart. The second plate of glass was a very little inclined towards the first, so that the rays reflected from the posterior surface of the first and from the anterior surface of the second plate might interfere with the rays reflected at the anterior surface of the first and the posterior surface of the second. A screen cut off the rays reflected from both anterior or from both posterior surfaces of the plane-parallel glass plates. The rays admitted by the screen fell upon a flint-glass prism with a refraction-angle of  $60^\circ$  and a vertical refracting edge, in which a spectrum having thick black bands of interference could be seen with the naked eye. By regulating the breadth of the slit and the distance of the same from the lens, it was possible to make Fraunhofer's lines clearly visible. The interference-bands were caused to fall parallel to Fraunhofer's lines by a proper adjustment of the plane-parallel plates of glass.

In the following experiments, both interfering rays of light were inclined at an angle of  $45^\circ$  or  $30^\circ$  to the anterior surface of the plane-parallel glasses, and had a great intensity in this arrangement of the apparatus. A very slight difference in the path of the rays, even when it fell short of  $\frac{1}{20}$  of an undulation, could be recognized with certainty by an alteration in the position of the thick interference-bands—especially when this alteration was restricted either to the upper or to the lower part of the spectrum, the other portion remaining unaltered. Besides, the displacement could be observed against the Fraunhofer's lines.

The section of each of the interfering rays (or, more correctly, bundles of rays) was very nearly a rectangle, the longer side being vertical. In the path of both bundles of rays, which were about 4 to 6 millims. apart, there was now placed a plane-parallel glass plate, on which there was a rectangular transparent metallic film. One side of this film was placed horizontal, the other vertical, so however that the latter lay between the two bundles of interfering rays. Assuming that the upper part of the bundle of rays I. passed through metal, and that the upper part of the bundle of rays II. passed through air, then the upper part of the spectrum would be formed of rays which would have a difference of path proportional to the thickness of the metallic plate, if light travels through metal with a velocity different from that at which it travels through air.

Since the metallic plate, and hence the upper part of the spectrum, was sharply bounded by a horizontal line, it was possible to observe the situation of the interference-bands in the lower part

of the spectrum with precision. Inasmuch as all the rays passed through the glass plate bearing the metal, they would all be retarded thereby in a similar manner. Indeed the above-described arrangement of the experiment even allowed the displacement of the interference-bands due to the metallic films to be estimated independently of the errors of the plane-parallel glass plates, so that one might even employ a piece of very good common mirror-glass.

Two other plane-parallel glass plates were also placed in the path of the interfering bundles of rays; these were fastened to the horizontal axis of a goniometer, in such a way that their refracting surfaces formed a small angle with one another, and cut one another in lines parallel to the axis of the goniometer, perpendicular to the direction of the interfering rays.

The ray I. went through the one, the ray II. through the other of these plane-parallel glass plates, which were thus differently inclined to the two rays. The paths traversed by the two rays through the plates were equal in one particular position of the axis of the goniometer; on turning the axis towards the right, the thickness of one of the plates increased faster than that of the other; on turning it towards the left, the opposite took place. Thus the pair of plates formed a compensator of Soleil and Duboscq's construction\*, such as Jamin has already employed in experiments with an interference-apparatus.

One is able at will, by means of this compensator, to retard the ray I. or II. to a slight extent, and so to destroy the displacement of the bands of interference in the upper relatively to those in the lower part of the spectrum, a displacement produced by the difference between the velocity of the rays passing through metal and through air in the upper part of the spectrum. If, in order to neutralize this displacement, it were necessary to retard the ray passing through metal, then the velocity during the passage through metal would be the greater; if the ray passing through air required retarding, then the velocity in air would be the greater.

In the whole of these experiments the transparent metal plate was placed nearly perpendicular to the interfering rays, in order that a displacement of the interference-bands due to the elliptical polarization of the light passing through the metal might be avoided.

Silver, which was transparent and had a blue or violet colour, or gold, transparent and of a brown or blue-green colour, effected a displacement which was destroyed by a retardation of the ray

\* See Jamin, *Ann. de Chim. et de Phys.* (3) vol. xlix. (1857) p. 288; and vol. lii. (1858) p. 166.

which passed through the metal; hence the velocity of light in these metals is greater than the velocity in air.

Gold may be investigated without the use of any support of glass, if a rectangular piece of gold-leaf be suspended vertically in the air. The experiment gives the same result either with or without glass.

Since, in order to be transparent, the metallic films must not be thicker than about one-fifth the length of an undulation, it follows that the displacement of the interference-bands never exceeds 0.1 or 0.2 of the interval between the interference-bands.

But, as Faraday has shown, silver and gold occur in very different modifications. Transparent silver of a yellow or grey colour causes a displacement which is neutralized by an acceleration of the ray passing through the metal; that is, the light passes with less velocity through the metal than through air. The same is the case with many varieties of gold and gold-leaf.

Gold and silver metallic plates, through which light passes with a greater velocity than through air, become modified by simple standing in the air, so as to transmit light with less velocity than it is transmitted by air. This should excite the less astonishment, since Vogel\* has noticed a spontaneous alteration of silver, and found that the mere contact of acids which occasion no chemical action turns the blue colour of silver immediately into grey or yellow. Yellow silver behaves towards reflected light also differently from blue silver. Since, moreover, the yellow varieties of silver, and the red or orange varieties of gold, become transformed by pressure into those which are transparent and blue or blue-green in colour, it follows that polished silver or gold must display the properties of the blue silver or blue-green gold, and have an index of refraction  $n < 1$ . This accords with the result at which Beer and Eisenlohr have arrived by applying the theory to the observations of Jamin on the light reflected from polished silver.

With transparent platinum plates there was always a displacement of the interference-bands of the spectrum which could be destroyed by an acceleration of the ray passing through the metal; *i. e.*, light passes through platinum with less velocity than through air.

The observations of the author upon the principal incidence A and azimuth H of the light reflected from polished gold or platinum plates, gave values from which, with the aid of Eisenlohr's formulæ, there came out  $n < 1$  for gold, and  $n > 1$  for platinum, as is shown by the following Table, in which the former notation is retained:—

\* Pogg. *Ann.* vol. cxvii. p. 318.

† *Ibid.* vol. civ. p. 374.



	A.	H.	n.	g.
Platinum .....	76° 51'	31° 28'	1.9493	3.6097
Gold .....	70 48	42 19	0.2705	2.531

$$\alpha = 45^\circ$$

Owing to the imperfections of the Nicol's prisms employed, in which the incident and emergent rays were not parallel, the values of A and H are affected by small errors.

The transparent brown gold-leaf was prepared by spreading a solution of gold, such as is employed for gilding in the porcelain manufactories, on a piece of looking-glass and heating it in an alcohol flame. In this instance, as also in the case of the platinum films, the glass support was strongly heated and then badly annealed. In order to eliminate the error arising from this, the metallic films were detached from the glass plates by the employment of a method of E. Mitscherlich's\*, consisting in immersing the plates in pure hydrochloric acid and then bringing them into contact with pieces of zinc. The zinc with the other metal formed a galvanic pair, and the hydrogen which was thereby disengaged at the surface of the gold detached the latter from the glass and it swam in the liquid. The liquid could then be poured out with care, and replaced by distilled water. Having washed the metal free from every trace of hydrochloric acid, it was placed on a plane-parallel plate of polished glass, freed from distilled water by evaporation, and finally lay flat on the glass surface. By using a clean knife it was easy to give to the film the requisite rectangular form.

A determination of the thickness of a silver plate, and of the displacement which it produced in the interference-bands, gave the index of refraction  $n = 0.5$ . Since, however, in the determination of the thickness of the metallic plate the specific gravity of the silver is assumed to be known, and it may vary, we must look upon this number as an approximation only.

However, the method above described does not admit of a very accurate determination of the indices of refraction of metals, since only very thin films of metal can be employed. This method, however, is independent of the presence of holes in the metal; for these affect only the distinctness of the interference-bands.

The displacement of the interference-bands might, however, arise from air which is condensed at the surface of the metal or the glass. Although a very great absorption of air at the surface of solid bodies must be assumed in order to explain the observed displacements of the interference-bands, nevertheless

\* E. Mitscherlich, *Lehrbuch der Chem.* 2 Aufl. 1840, vol. ii. p. 340.

the author has repeated the experiments *in vacuo*. The plate of glass with the film of metal, or the free suspended gold-leaf, was in these experiments contained in a brass box which was closed in by two plane-parallel and accurately parallel plates of glass. By means of a cock the interior of the box, dried by means of bits of chloride of calcium, was placed in communication with the air-pump and exhausted. After exhaustion, the height of the barometer was 4.5 millims.

The appearances were the same in the atmosphere and *in vacuo*. This, therefore, was the experimental proof that the velocity of light in the above-named modifications of gold and silver is greater than *in vacuo*, whilst in platinum it is less.

Were the transparent metallic plates inclined towards the transmitted rays, and these, therefore, not at perpendicular incidence to the same, then the conditions of the experiment become very complex. For, at the same time, the thickness of the plate is augmented, and thereby the difference of velocity of the rays passing through metal and air is increased, and the consequent displacement of the interference-bands in the spectrum is increased also; and further, if Cauchy's theory be true, the index of refraction would increase with an increasing angle of incidence, which in the case of metals which at perpendicular incidence have an index of refraction  $n > 1$  would bring about an increase, and in the case of those which have  $n < 1$  would bring about a decrease in the displacement of the interference-bands. Indeed one sees, from the form of the expression for  $\nu$  (equation 4), that the index of refraction in the latter metals may become  $> 1$ , and then, for the angle of incidence at which this takes place, the displacement of the interference-bands must pass into one in the opposite direction.

To this must be added the displacement which is due to the elliptic polarization of the light which passes through metals. For light which is polarized in the plane of incidence of the metal, there is produced, according to what has been said before, an acceleration in relation to the light polarized perpendicular to the plane of incidence of the metal. The displacement of the fringes in the instance of metals having an index of refraction  $n > 1$  will therefore be greater for light polarized perpendicular to the plane of incidence, than for light polarized parallel to the same.

Experiment supported this conclusion perfectly when the metal plates with the glass which carried them were placed obliquely against the incident rays. When the interference-bands of the spectrum were observed through a Nicol's prism, the displacement for light which was polarized parallel or perpendicular to the plane of incidence of the transparent plate of

metal could be determined. It was observed that both in the case of transparent gold and of transparent silver plates, for which  $n > 1$ , the displacement of the interference-bands (in the sense of retardation of the rays passing through metal), for light polarized perpendicular to the plane of incidence of the metal, increased with increasing incidence, and under an incidence of  $85^\circ$  amounted to 0.3 of the space between the interference-bands. For light polarized parallel to the plane of incidence of the metal, the displacement was 0, or at most 0.1 of the space between the bands, in the sense of a retardation of the ray passing through metal.

Thence it would follow, regard being had to the above, that, for rays of light passing through metal, the component polarized parallel to the plane of incidence is accelerated relatively to that polarized perpendicular to the plane of incidence, and that the acceleration increases with increasing incidence. This agrees with the above-given experiments upon the elliptical polarization of the light going through metals, the experiments having been made with Babinet's compensators. These experiments leave it in uncertainty whether the component polarized perpendicular to the plane of incidence is retarded continuously with increasing incidence, or whether the component polarized parallel to the plane of incidence is continuously accelerated. Now, since the greater displacement of the interference-bands for light polarized perpendicular to the plane of incidence arises from the superior thickness of the layer of metal passed through at large angles of incidence, it is apparent that the component polarized perpendicular to the plane of incidence suffers a smaller alteration of phase on account of simple refraction than the component polarized parallel to the plane of incidence, and that this alteration of phase for the last component increases with increasing angle of incidence.

This circumstance admits of the same considerations in the case of transmitted or refracted light as in that of reflected light, with which the author, in a former communication\*, sought to estimate the position of the oscillations of the particles of the luminiferous æther in relation to the plane of polarization. For the oscillations which take place perpendicular to the plane of incidence, the position of the paths of the luminiferous particles remains the same towards the refracting surface when the angle of incidence is altered. But in the case of the oscillations which lie in the plane of incidence (and like the former are perpendicular to the ray of light) the inclination of the path of the lumi-

\* See *Monatsber. d. Berl. Akad.* 1862, p. 714. *Pogg. Ann.* vol. cxviii. p. 445. *Phil. Mag.* S. 4. vol. xxvi. p. 190.



niferous particles towards the refracting surface increases with an increasing angle of incidence.

In vertical incidence, the rays polarized perpendicular to the plane of incidence must be affected just like the rays polarized parallel to it, since in both the oscillations of the æther lie in the refracting surface. For oscillations perpendicular to the plane of incidence, there is no, or only a very slight difference of phase produced by different angles of incidence; but it is otherwise in the component having oscillations parallel to the plane of incidence, and in which the inclination of the paths of the luminiferous particles towards the refracting surface alters.

Now, since these researches show that this alteration is greatest in light polarized parallel to the plane of incidence, and increases with increasing angle of incidence, this property of light transmitted through metals leads to the same conclusion as the properties of light reflected from metals, namely that the oscillations of the luminiferous æther follow the plane of polarization.

Finally, it may be remarked that, by means of Babinet's compensator, the author was unable to detect any difference of phase in the components, polarized parallel and perpendicular to the plane of incidence, of the light which had previously passed through transparent substances such as plates of glass. Therefore the analogy between metals and transparent bodies, which Jamin has proved for reflected light, is not maintained in relation to transmitted light.

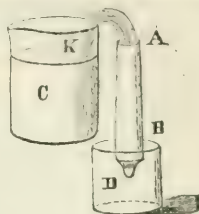
XXX. *On the Magnitude of a Drop of Liquid formed under different circumstances.* By T. TATE, Esq.\*

PEOPLE generally, and medical men especially, speak of a drop of liquid as if it were some definite quantity, whereas, as the results of the following experiments show, its weight varies not only with the diameter of the tube in which it is formed, but also with the density, temperature, and chemical composition of the liquid. The particles of a liquid are retained in the form of a drop by the force of adhesion subsisting between these particles and the sides of the tube, or the substance on which the drop is formed; and opposed to this force is that of gravity and the cohesion amongst the particles of the liquid: when the drop attains a certain magnitude, the force of gravity, acting upon it, counterbalances that of adhesion, and then the liquid falls. Any change of conditions, therefore, affecting these forces must influence the magnitude of the drop. Under the same circumstances, the weight of a drop of liquid, as determined by experiment, is remarkably uniform, not varying in any

\* Communicated by the Author.

case more than the hundredth part of itself. All substances capable of being wet by the liquid give a drop of the same magnitude.

These experiments were for the most part made with the following apparatus:—C represents the vessel containing the liquid



to be experimented upon; K A a strip of clean calico dipping into the liquid and partially plugging the lower extremity of the tube, A B, fixed in a vertical position, delivering the drop D into a light beaker B. The strip of calico, acting on the principle of a siphon, may be made to convey the liquid into the tube with any requisite degree of slowness by adding to, or taking from, the liquid in the vessel C. In the following experiments, the adjustment was such that the interval of time between the falling of any two consecutive drops was not less than forty seconds. The lower end, B, of the dropping-tube was formed into a sharp edge, so that the tube at the part in contact with the liquid might be regarded as indefinitely thin. From five to ten drops of the liquid were allowed to fall into the beaker, and then its augmentation of weight enabled me to find the weight of each drop. The form of the drop is that of a hyperboloid of double curvature, approaching a hemisphere at the lower portion, concave towards its upper portion, and having a contrary flexure about the middle. In order to determine the weight of the drop corresponding to different temperatures, the dropping-tube A B was inserted in a wide brass tube passing through a hot-water bath, so that the drop of liquid, as it was being formed, might attain the same temperature as the water surrounding the wide tube.

*Other things being the same, the weight of a drop of liquid is proportional to the diameter of the tube in which it is formed.*

This remarkable law seems to be fairly established by the following results of experiment.

TABLE I.—Results of Experiment on the Weight of a drop of Water, formed in thin tubes of different diameters, at constant temperature 50°.

Diameter of tube, in inches, D.	Corresponding weight of drop, in grains, w.	Value of $w$ by formula $w = 5.5 D.$
.13	.78	.71
.25	1.36	1.37
.30	1.60	1.65
.39	2.10	2.14
.50	2.84	2.75
.56	3.00	3.08
.62	3.36	3.41

Here the near coincidence of the results in the second and third columns shows that the weight of the drop is in proportion to the diameter of the tube.

When the diameter of the tube exceeds seven-tenths of an inch, the continuity of the liquid seems to be broken by the agitation occasioned by the falling of the drop, and then the whole volume of the liquid in the tube falls, but not in the form of a regular drop.

*The law just enunciated has a remarkable relation to that of capillarity.* Since the height to which water rises in a tube, by the action of capillarity, varies inversely as the diameter of the tube, it follows that the weight of water rising in any tube from this cause varies directly as the diameter: hence *the weight of the drop is in proportion to the weight of water which would be raised in that tube by capillary action.*

When the liquid drop is formed upon a flat circular surface, the law expressing its weight assumes a more general form. In this case it was found that the augmentations of weight were in proportion to the diameters of the circular surfaces. In these experiments, the surface on which the drop was formed was the horizontal base of a solid cylinder of hard wood, the liquid being conveyed by the calico strip uniformly over the exterior surface.

TABLE II.—Results of Experiment on the Weight of a drop of Water, formed on circular surfaces of different diameters, at constant temperature 50°.

Diameter of surface, in inches, D.	Corresponding weight of drop, in grains, <i>w</i> .	Value of <i>w</i> by formula $w = \cdot 22 + 2\cdot 7 D$ .
·1	·41	·49
·2	·76	·76
·3	1·10	1·03
·4	1·32	1·30
·5	1·56	1·57
·6	1·78	1·84
·7	2·15	2·11

Here the near coincidence of the results in the second and third columns shows that the formula  $w = \cdot 22 + 2\cdot 7 D$  very nearly expresses the weights of the drops: from this formula it follows that *the augmentations of weight are in proportion to the diameters of the surfaces on which the drops are formed.*

The drop given off from the circular surface whose diameter is seven-tenths of an inch is the greatest that can be formed in this manner, as will appear from the result of the following experiment.

A circular horizontal surface of indefinite diameter gave a  $\frac{7}{10}$  drop



of water weighing 2.10 grains. This is therefore the weight of the maximum drop formed on a flat surface, and it will be seen that it very nearly coincides with the weight of the drop formed upon a surface seven-tenths of an inch diameter. The drop in this case is always formed at the centre of the surface, this being the centre of the greatest molecular attraction amongst the liquid particles.

It will be observed that the drop formed in this manner is considerably less than that which is formed in the tubes, the reason of which is too apparent to require any explanation.

*The weight of a drop of liquid, other things being the same, is diminished by an augmentation of temperature.* With a tube .3 inch diameter, the weight of a drop of water at 40° F. was found to be 1.65 grain; whereas at 150° it was found to be only 1.45 grain, that is, the decrement of weight due to 110° was one-fifth of a grain; and so on to other temperatures. The decrements of weight due to the temperature T were found to be closely expressed by the formula  $.0018(T-40)$ ; so that the weight of a drop of water, at different temperatures, formed in a thin tube of D diameter will be expressed by the formula

$$w = \{5.5 - .006(T-40)\}D.$$

*Independently of density, the chemical composition of a liquid affects the weight of its drop in a remarkable manner.* Most acids, alkalies, and alkaline salts diminish the weight of the drop. A little potassa added to distilled water sensibly reduces the weight of the drop. Strong sulphuric acid, having a density about  $1\frac{3}{4}$  times that of water, under the same circumstances gives a drop considerably less than that of water. Nitric acid, with a density about  $1\frac{1}{2}$  times that of water, gives a drop of about four-fifths of that of water. Spirits of wine, having a specific gravity of .957, gave a drop of only about one-half of that of water; and this spirit, diluted with sixteen parts of water, making its density very little less than that of water, gave a drop weighing less than six-sevenths of that of pure water. Olive oil, having a specific gravity of .9176, gave a drop of only about one-half of that of water. Liquids having a low degree of fluidity and a high cohesive attraction amongst their particles, such as molasses, solutions of soap, starch, &c., all gave drops considerably less than that of water. Hence it appears that where the molecular force of cohesion is greatest, *ceteris paribus*, the weight of the drop is least, and conversely.

The weight of the drop of pure water being 2.84 grains, the weights of a drop of the following solutions, having the same specific gravity, viz. 1.190, were found to be,—chloride of sodium, 3.30 grains; solution of sugar, 3.06 grains; carbonate of soda,

3·00 grains ; sulphate of magnesia, 2·97 grains ; nitric acid, 2·58 grains ; sulphuric acid, 2·30 grains.

*In different solutions of chloride of sodium (and other neutral salts) the increment or augmentation in the weight of the drop is in proportion to the weight of the dry salt in solution.*

TABLE III.—Results of Experiment on the Weight of a drop of different solutions of Chloride of Sodium at constant temperature 50°.

Per cent. of salt in solution, <i>r</i> .	Corresponding weight of drop in grains, <i>w</i> .	Value of <i>w</i> by formula $w = 2\cdot84 + \cdot0135r$ .
0	2·84	2·84
8·5	2·93	2·95
17	3·07	3·07
34	3·30	3·30

The coincidence between the results in the second and third columns confirms the law above enunciated.

Hastings, January 20, 1864.

XXXI. *On the Formation of the River- and other Valleys of the East of England.* By S. V. WOOD, Jun.\*

[With a Map, Plate I.]

THE valleys of the East of England contain within themselves so much evidence to show that their formation was synchronous, and due to the same series of events, that I am induced to attempt to trace their origin to its source. My object is briefly to submit the evidence upon which I have formed the opinion that the whole of this valley system has originated from the denudation having been induced and regulated by earthquake disturbances that centred in two foci, one of which lies immediately without the Valley of the Weald, and the other in the sea off Brook Point in the Isle of Wight, both of them having taken place while the sea of the upper drift covered the island.

A careful study of the ordinary Ordnance maps will show that the whole of the inequalities of surface in that part of England which lies east of a line drawn from the Humber to the Cotteswold Hills are divisible into three distinct groups. The first of these groups consists of a regular series of circles diverging from a centre, as the circles of undulation diverge from the point where a stone is dropped into water. The centre of these circles lies about ten miles south-east of Canterbury, and from it these concentric circles emerge at tolerably regular intervals,

\* Communicated by the Author.

until they cease abruptly against the outcrop of the Trias. The innermost of these circles is completed in England by the Dover and Folkestone cliffs; but of the others, a less and less portion of the circumference falls within England, as the circles become larger from emergence. The second class of inequalities is in all respects, except in the degree of force by which they have been produced, similar to the first; but the circles from which they result emerge from a centre lying in the sea a few miles south-west of Brook Point, in the Isle of Wight. The circles of both groups are broken into an irregular outline, which partakes of the form of a series of springing curves. The circles of the second group are none of them completed within England, not more than half their circumference falling within it.

The third group consists of inequalities having a general parallelism to each other, and which appear as repetitions of the scarp of the North Downs between Maidstone and Ashford. I shall endeavour to show that they are the result of flexures produced in the strata by the pressure of the Isle of Wight circles on the beds first disturbed by those from the Kentish centre.

The circles of the second group present features differing in some respects from those of the first; for although those of the first may be traced, almost unimpaired in regularity, through a part of the proper area of the second group, yet in other parts the latter exhibit the appearance of having operated upon the strata moved by the first group, and warped them into that form which characterizes the third group of inequalities in the East of England. The inequalities resulting from this interference become more and more minute, but yet more frequent, as the distance from the two centres increases, until in Norfolk, and also along the verge of the Triassic outcrop that stops them in the Midland Counties, their intricacy becomes extreme.

These three groups form all the valleys of the East of England; and, so far as I have been able to test them, these valleys are entirely alike in the evidence they present as to the date of their origin. They all cut through the drift wherever it occurs: that is to say, where the lower drift alone has been spared by the denudation, they cut through that; where the upper or clay drift remains covering the lower, they alike cut through both drifts; and where the upper drift rests upon the Eocene, or the Secondaries, they cut through that and the underlying tertiary or secondary bed.

It requires a minute and careful study of the maps of the General Ordnance Survey to reduce these three groups of inequalities into the harmonious outline that they actually present; and for the purpose of making them conspicuous, I have carefully extracted into the Map (Plate I.) accompanying this paper all



the more deeply marked inequalities of surface. With one or other of these three groups of inequalities, all those too feeble or minute to be included in the Map are identical in contour. I have represented each of the two first groups by a distinct shading, and those of the third by the shading of that one of the first two from the influence of which they have resulted. It would be more easy to attract attention on the Ordnance Maps to the regularity of these circles, were those from the Isle of Wight centre taken for examination, as the greater power exhibited by that series, and the absence of the Wealden denudation near their centre, render them conspicuous to the most cursory observation; while either from the greater denudation that has supervened upon the Kentish circles, or from the less force with which they were generated, or probably from both these causes, the successive circles are in some parts of their arcs only to be traced by the course of the water-drainage.

A glance at the annexed Map will show with what remarkable regularity the outline of the innermost of the Kentish circles, forming the plateau of North-eastern Kent, is repeated in all the exterior circles, even to the outermost, although the arcs, as they emerge, break gradually into a series of springing curves.

I will now give for illustration one or two of the repetitions of the curves in forming the valleys of the East of England.

If a rod be laid across the Ordnance Map from Wrotham (which is the innermost salient point of the springing curves into which the circles are broken, that the great denudation of the Weald has left sufficiently distinguishable) to the nearest corresponding salient point in one of the outer circles at a place called on the Map Peakirk Moor, three miles south-west of Market Deeping, a distance of ninety miles, it will intersect nine of these arcs at the corresponding salient point in each, or within a mile on either side of the rod. These points are, the angle of the Thames at Greenhithe; that of the Roding at Albyns near Stapleford; that of the Stort at Pishobury near Sawbridgeworth; that of the hills near Royston at Chishall; that of the Cam at Meldreth; that of the Bourne Brook at Bourn near Caxton; that of the Ouse at Hemingford Grey; that of the Nen at Peterborough; and that of the Welland at Peakirk Moor. Similarly, if a rod be laid about twelve miles west of the last, from Chevening (the next salient point along the Weald to Wrotham) to the next salient point of the Welland to that in the last case, which is at a place called Duddington, also a distance of ninety miles, it will cut the salient points at the following places: the angle of the Thames at Woolwich; that of the concurring valleys of the Lea and Roding at Woodford; that of the Lea at Hertford; that of the Royston Hills at Clothall; that

of the Ouse at Tempsford; that of the Nen at Oundle; and that of the Welland at Duddington.

A long list of such concurrences might be adduced; but my object being brevity, I refer to the annexed Map, and invite the closest and most rigorous comparison of it with the Ordnance sheets: it is an exact epitome of what will be found there, only that the faint or obscure inequalities and the conspicuous ones are necessarily represented by me alike. The continuation of the arcs through the fens may even in that apparently dead level be distinctly continued or connected by the true test of the river-beds there.

Although the valleys of the more eastern part of the area mapped derive their force mainly from the denudation that has eroded them, yet it will be found that this denudation has operated according to the direction imparted to it by the configuration of surface that I have been describing; and before adverting to the denudation, I will therefore advert to the causes that have apparently produced this configuration. The aspect presented by both series of circles is such as a pasty or doughy mass spreading by its own weight would take, except that there the diverging circle would be singular, and not repeated successively as we have it here. It appears to me that we have here the result of the yielding of soft strata to the lateral force, caused by the protrusion of a boss of matter from beneath the respective circle-centres. The annexed hypothetical sections of each series will show my meaning. The lateral force exerted would be proportional to the amount of strata elevated and thrown over. Upwards of 4000 feet of strata have been elevated over the Isle of Wight centre; but the greater part of that has resulted from the subsequent and rectilinear movements there that I shall presently notice. On the other hand, we cannot accurately estimate the amount over the Kentish centre: assuming, however, that the whole thickness of the Eocene present on the Essex side of the Thames was once recumbent over this centre of elevation, the thickness of the displaced strata will not greatly exceed 1000 feet above the sea-level. The lateral pressure thus exerted may be illustrated by the rumpling up of a table-cloth. On the first pressure a wrinkle arises, which reaches its maximum before the next beyond it commences; and that process is repeated as each wrinkle onwards arises; the greater the rigidity of the cloth, the more speedily do the more distant wrinkles appear. Supposing that pressure to radiate from a centre, the wrinkles would emerge as circles broken as we see those in Somersetshire and Wiltshire. The difficulty in applying such an action to strata consists, among other things, in their immobility and varying rigidity.

Inasmuch as the lateral displacement should be greatest in the case of the uppermost beds, from the greater length of the exterior curves over a dome of elevation to those more interior, as shown by Mr. Hopkins in the case of the Weald, the greatest results would be looked for where the uppermost beds were present. But against that we have the greater mobility of the soft sandy Eocene strata over the more rigid chalk; so that the latter, with a less displacement than the former, might produce at greater distances equal or even greater flexures than the latter. The same observation applies equally to the varying rigidity of the lower cretaceous and oolitic beds. We have a concurrence of evidence to show that these movements took place when the sea of the upper drift covered the island; and it would be a subject of much interest to discuss how far the influence of a superincumbent sea would conduce to an increase in the pasty or pliable condition of the strata beneath it, did space permit me here to enter on that subject. I will only observe, therefore, that it appears to me that this influence would be great, since, if the impervious clays did not completely overlap the sandy beds, the pressure of the water must reduce the porous beds of sand to the condition of quicksands, by filling every interstice between the grains with water. We know also how great is the quantity of water that chalk will as land absorb; and this quantity may be capable of large increase under the pressure of a sea, and the pliability or pasty consistency of the mass proportionately augmented. These are the causes to which, as it appears to me, the inclination of the oolitic and cretaceous strata, that in sections attached to early geological maps were represented as arising from an upheaval in the north-west, is really due; that is to say, in lieu of such elevatory action from the north-west, they have been produced by a lateral pressure radiating from a centre in the South-east of England; and in support of that view I call particular attention to the manner in which the circles abruptly cease against the Triassic outcrop generally, and most conspicuously so where that outcrop, backed by the close neighbourhood of the palæozoics, presents most the character of a wall stopping the outspread, as in the region skirting the eastern side of the Somersetshire coal-field.

It should of course follow, if these views are sound, that all the ridges which the surface presents should be alternations of anticlinal and synclinal, however faint these may be. Much of the valley system of the East of England consists of lines of hills produced by denudation, as the chief and almost the only apparent cause, so that it is not easy to determine whether these lines of hills are really anticlinal. On the coast, where the hills are in section, the alternation of anticlinal with synclinal



can be shown, and it there exists; and so far as I have been able to test the inland hills, the same thing exists there also invariably. Along the Suffolk and Norfolk coast short faults and the broken condition of the strata attest the pressure produced by the North Sea circles opposing those of the Isle of Wight; and inland similar faults and breaks frequently occur.

The operation of the denudation has been to deepen greatly all the synclinals, leaving the crests of the anticlinals in some cases (most particularly in the soft strata), where the drift clay remains on them, almost intact; and in others (and most particularly where hard strata are operated upon) to remove the broken mass occurring over the line of greatest flexure, leaving the rest of the anticlinal but little denuded. That would be the result of a sea bed, wrinkled as I have described, emerging from the sea. The crests of the anticlinals, once clear of the water, would be safe from denudation; but that action would continue in the synclinals until the land was altogether free of the waters. It should also follow from this that the denudation has been greatest where the troughs have been deepest; and if we omit the immediate centres of the circle, where the denudation has been greatest, from the greater fracturing, and omit the actual Valley of the Weald, which with the southern edge of the isles of Purbeck and Portland and Wight has undergone a separate and local upheaval and denudation, subsequent, as I conceive, to that now considered, we shall find that the denudation has followed that law. The greatest inequalities have (taking equal distances from the centres) been produced where the two series of circles have come into direct conflict, and the least where the angles made with each other are greatest; and comparing soft beds with soft beds and hard with hard, the amount of denudation has coincided with the extent of those inequalities. Compare, for instance, the space between Bagshot and Chobham ridges (which are arcs of the Isle of Wight circles), and the heights of Hampstead (which are arcs of the Kentish circles), there diametrically opposed to each other, as an example of extreme denudation, with the area of High Essex, where the arcs of the two series cut each other at right angles, as an example of minimum denudation. In the one case the whole mass of the Bagshot series and part of the London clay has been removed. In the other the Eocene series, with the drift beds overlying it, remains intact. The New Forest is another example of minimum denudation accompanying the cutting of the two circles at high angles; and but for the subsequent local denudation of the Weald interfering with the problem, the area of the South Downs would probably have afforded another conspicuous example of the opposite state of things.

The presence of arcs of the Kentish circles, almost uncontorted, among the Isle of Wight arcs near to their centre in Hampshire, and occupying their normal position, while the Isle of Wight arcs in Essex and the rest of East Anglia, bearing the same relative position to those of the Kentish series, are warped into a double flexure, would appear inconsistent. I, however, entertain no doubt that a third series of circles has emerged from a centre in the North Sea, about 70 miles east of Flamborough Head, and that it is to them that the hills of North-east Lincolnshire are due, as well as those ranging from Lincoln past Grantham to Stamford. The pressure from these circles operating almost, but not quite, opposite to those of the Isle of Wight series has caused the *double flexure* possessed by the third or invading group of inequalities. The conflict of this third series with that of the Isle of Wight has also produced the confused condition of the Norfolk valleys; but the most remarkable result of that conflict is the contorted and intermingled condition of the cliffs of North-east Norfolk, which occurs most at the point where the arcs are directly opposite to each other (*i. e.* between Cromer and Sherringham), disappearing as the angle made by the arcs crossing each other increases. The same cause has also produced the forcing up of the chalk inliers of Sudbury and Claydon in Suffolk. In the absence, however, of the same satisfactory means of tracing from their centre this third series of circles that exists in the case of the other two, I have for the present left such of the arcs belonging to them as do not oppose those of the Isle of Wight series, shaded in the Map as though belonging to the Kentish series; and such as do oppose and form a compound or double flexure, shaded as though belonging wholly, instead of (as they really are) in part only, to the Isle of Wight series\*.

The influence of these circles upon the Valley of the Weald demands a special though brief notice. It will be seen that the openings in both the North and South Downs, that were regarded by Mr. Hopkins as the result of fissures caused by the doming of the strata along the rectilinear elevation of the axis of the Weald Valley, do not occupy the position that on such an hypothesis they should do; instead of being rectilinear, as fissures so caused should be, they are curvilinear. They, it will be found, form, as to some of them, regular and corresponding fragments of

\* It would be more correct to describe these invading inequalities as the result of the squeezing together of the strata by the North Sea circles opposing those of the Isle of Wight, than, as is done in the text, to describe them as belonging to the latter series. The North-east Lincolnshire hills are inaccurately represented in the annexed Map; their direction should be concave towards the sea, instead of the reverse.

arcs of the Kentish series, with precisely the curvature that their position in those circles should present; and as to others, they are similarly exact portions of the arcs of the Isle of Wight circles. A close examination of the common Ordnance sheets will disclose these features more decidedly than on the small scale of the annexed Map it is possible to distinguish them. Further, these sheets will show other fragments of these arcs over the centre of the Weald Valley, preserving their distinct character through the enormous denudation to which that valley has been exposed; and I invite this close examination by the light of the annexed Map as a guide.

It appears to me, from the evidence of the great denudation in the Weald Valley (which has, with these fragmentary exceptions, destroyed the plications of the circles crossing that valley) having accompanied the rectilinear upheaval which is independent of the curvilinear plications that I have been describing, that such rectilinear upheaval was of subsequent date to the operations I have been discussing. The operation of that local denudation has been to denude and extensively remove the arcs crossing the valley, and to deepen into their present state the gorges through which the rivers of the Weald escape to the sea; and it has therefore been undoubtedly posterior to the formation of those arcs. And as the rectilinear elevation has, as it would seem, been contemporaneous with that local denudation, I cannot but regard the denudation which has given its present form to the Weald Valley as subsequent to that forming the valleys of the rest of the East of England,—the denudation over the East of England having taken place when the island (then or soon after a peninsula) emerged from the drift sea, and having ceased over the greater part, which had become land, some time prior to the cessation of the local denudation. This rectilinear upheaval and local denudation I regard as contemporaneous with the formation of the higher (and perhaps of a part of the lower) level gravels that fill the valley of the Thames, and which formed in that valley the transitional stage between the events that constitute the principal subject of this paper and the present state of things—a stage when the isthmus connecting the peninsula of England to the main was skirted by the estuary occupying the whole gravel area of the Thames Valley on the north, and by the estuary or inlet of the Weald Valley on the south, and when yet another deep inlet occupied the area in Hampshire now covered by the superficial gravel of Christchurch Bay, which ranges up to the verge of the great rectilinear ridge that borders it on the south.

I believe also that abundant evidence exists to show that the elevatory action which commenced with the convulsions I have traced on the Map, and which by its sustained but diminished



and more slowly acting force brought the anticlinals above the sea, and from stage to stage converted the gravel-beds into the low hills of the valleys, and the channel-bottoms into marsh, reducing those channels to the narrow courses in which they now run, is still in operation, and that it has produced appreciable results in elevation even in the short period that has elapsed since the Roman invasion.

To prevent misconception, I would also observe that the same rectilinear movements which in the Weald Valley supervened on the circular divergences described, appear to me to have also supervened on the same divergences in the south of Hampshire and Dorsetshire, by which much of the elevation there appearing at first sight as due to the focus of the Isle of Wight circles has been added to the elevation that introduced these circular phenomena, and that it is by a combination of the two that the present highly inclined condition of the central line of that isle, and the consequent enormous denudation over that line, have resulted. I have endeavoured to represent the effect of this compound action in Section I (Pl. I.), in which all the beds from the Wealden upwards are represented in the position they now actually occupy; but the tertiaries from the base of the Headon series upwards are connected by dotted lines with the arc of upheaval, of which they still remain an almost undisturbed part, the tertiaries beneath that series, together with the cretaceous beds, having been brought into their present almost vertical condition and squeezed against the Headon series by the subsequent rectilinear movements referred to. It may be a startling reflection to the residents of some portions of the south coast, that the places on which their towns stand have, as I believe, been upheaved sharply and locally to an extent of between 2000 and 3000 feet by these rectilinear movements *during and since* the time when the men whose remains occur in the high-level gravels of the Somme dwelt on the banks of that river. Further, the phenomena disclosed by the Map seem to point to the formation of the two series of circles as not absolutely simultaneous, but that some interval, although possibly of the very shortest or momentary duration, elapsed after the upheaval of the Kentish and before that of the Isle of Wight centres.

If the foregoing views are well founded, the results concur with the inference I have drawn from, amongst other evidence, that of the deposit of the upper and lower drift: viz., that over the East of England no irregularities of surface existed prior to the close of the drift epoch, other than those produced by erosion; and that, with the exception of the inequalities resulting from that cause, the entire surface covered by oolitic, cretaceous, and tertiary deposits remained, until the outburst of these convul-

sions, pretty nearly in the condition that their respective seas had left them,—the only exception, and that of a very moderate nature, consisting of the early tumefaction that began over the Wealden area prior to the deposit of the older tertiaries, as shown by Mr. Prestwich.

In concluding, I wish to withdraw an opinion expressed by me in a paper published in the *Philosophical Magazine* for April 1863, that the departure of the Wealden Valley between Maidstone and Dover from the linear east and west direction was due to the influence of a production of the palæozoic barrier of the Pas de Calais beneath that part of Kent. It is evident, from the views put forward in this paper, that such an opinion is no longer tenable by me. At the same time that I adhere fully to the other views there expressed as to the direction of the extension of that barrier, I can no longer attribute that departure to any other than the causes shown in the Map,—that is to say, to the rectilinear upheavals of the South of England having never reached in Kent further east than the verge of the innermost Kentish circle—a part of whose periphery forms the Dover extremity of the Downs between that place and Maidstone, while the other, or Maidstone extremity, is formed of a powerful example of the series of inequalities of the third group, caused by the invasion of the Isle of Wight circles into those of Kent.

I have little doubt, from the investigation that I have already been able to give to the subject, that the river- and other valleys of the secondary and tertiary basins of Northern France and of Belgium have been formed by the action of that portion of the arcs of the two series of circles which are (after allowing for the hiatus of the British Channel) complementary to those delineated in the coloured Map. I have not, however, yet had the time to perfect my evidence sufficiently to embody it in a satisfactory form.

P.S. Since the above went to press, the Map of the Geological Survey for the north-west side of the Weald has appeared. It shows the Isle of Wight arc made by Chobham ridges distinctly formed by the upper Bagshot brought out by denudation, precisely as I have by survey myself ascertained to be the case in the Kentish arcs in Eastern Essex, where the lower Bagshot is brought out over the London Clay by denudation. The Kentish arc running through Leith Hill and directly opposing Chobham ridge, is also brought out by the lower tertiary outliers upon the chalk that follow the curve. The continuation of the Kentish arc across the Weald is also to be followed by a ridge of Paludina limestone in the Weald clay. I look with interest to the future

Geological Survey Sheets for the light they will throw upon the relation of these subsequent rectilinear upheavals and local denudation to the previous circular movements and general denudation—and particularly to the Sheet that will comprise the innermost Kentish circle.

XXXII. *On the Traces of a Spiral Structure in the Solar System.*  
By EDWARD VANSITTART NEALE, Esq.\*

IN a paper published in the Philosophical Magazine for December last, I have pointed out a theoretical arrangement of the centres of planetary formation, whence I have endeavoured to explain the actual distances of the planets, upon the supposition that the solar system has grown up out of the condensation of a mass of material formerly occupying the area now traversed by the planets. For convenience of reference I repeat the Table containing the summary of my theory.

Group.	Names.	Theoretical distances.		Totals.	Actual distances.	Difference.	Proportion of difference to theoretical radius.
I.	Mercury ...	2		2	3.87	+ 1.87	+ .935
II.	Venus .....	2	+2	4	7.23	+ 3.23	+ .807
	Earth .....	2	+2× 3	8	10	+ 2.00	+ .200
	Mars .....	2	+2× 6	14	15.24	+ 1.24	+ .089
	Juno † .....	2	+2× 12	26	26.70	+ .70	+ .027
III.	Jupiter.....	2	+24	50	52.02	+ 2.02	+ .040
	Saturn .....	2	+24 +24× 3	98	95.39	− 2.61	− .027
	Uranus.....	2	+24 +24× 6	170	191.82	+21.82	+ .128
	Neptune ...	2	+24 +24× 12	314	300.37	−13.63	− .043

It will be seen, on inspecting this Table, that while it exhibits a symmetrical arrangement in the spaces occupied by each group of planets, and in the arrangement of the planets within each of these spaces, there is a want of symmetry at the points of transition from one group to the next in succession,—*i. e.* in the distances between Mercury and Venus, and between Juno and Jupiter, as compared with those between Venus, the Earth, Mars, and Juno, or Jupiter, Saturn, Uranus, and Neptune.

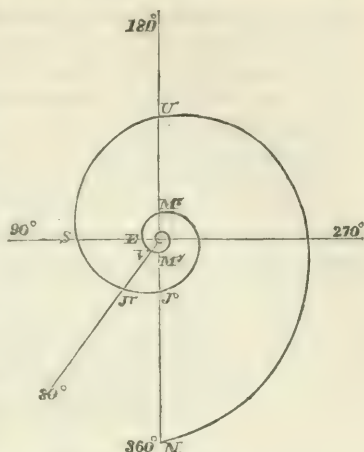
I wish at present to suggest a consideration which seems to account for this apparent anomaly. Lord Rosse's observations have disclosed a tendency to a spiral structure in many of the nebulae. Now if we suppose that the solar system was consoli-

\* Communicated by the Author.

† Juno is taken as the nearest representative of the probable centre of the group of asteroids.



dated from a mass originally possessing a spiral constitution, and that the planetary centres were arranged, at the relative distances stated in the Table, upon a plane, continuous, spiral line, making three coils, of which each had a breadth twelve times greater than its immediate precursor, we shall obtain the results shown in the accompanying figure—which, however, must be taken to illustrate only the angular position of the generative centres, and not their relative distances, since these could not be exhibited within the limits of the page\*.



It will be seen that upon this hypothesis we obtain the results following:—

1. The original position of the generative planetary centres in each coil of the spire would fall on the same angular points.

2. All these centres would have been placed in the same half of the constitutive mass; seven out of nine within the same quadrant; and five out of these seven within a segment of 30 degrees.

Hence the original mass must have been *decidedly overweighted* in one quadrant, and would thus have displayed from the first that principle of adaptation by which we find existence to be so deeply penetrated upon the earth. For such a disposition of the planetary centres must obviously have been adapted to secure the uniform rotation of the whole mass in which they were situated, in the direction determined by their mutual attraction and the pressures upon them, and thus to lay the foundation for that process out of which a system of planets circulating round a central sun could arise.

Since the publication of my former paper, my attention has been drawn to certain calculations by Mr. S. M. Drach, published in the *Philosophical Magazine* for January 1841, "On the Limit of distance at which a Planet could circulate round the Sun without danger of falling into it." Mr. Drach fixes this limit at 16,000,000 of miles. Now if my theory of the original position of the planetary centres be correct, there should have been

\* Supposing the innermost coil to have in its broadest part a radius of only  $\frac{1}{4}$ th of an inch, the second coil would have an additional breadth of inch, and the third of 6 inches.

three such centres between the Sun and Mercury, at distances originally  $\frac{1}{12}$ th,  $\frac{3}{12}$ ths, and  $\frac{6}{12}$ ths of that of Mercury. But of these, according to Mr. Drach's calculation, the two first must have been swallowed up by the Sun; so that we can expect to find one only, namely the third, which, though originally within the limit, would have been driven beyond it by the great expansive force arising from the heat generated in the later stages of the process of condensation. It is interesting that M. Leverrier should have recently expressed the belief that such a planet exists, and has been observed, between Mercury and the Sun.

### XXXIII. On some Properties of Ice.

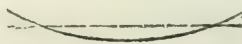
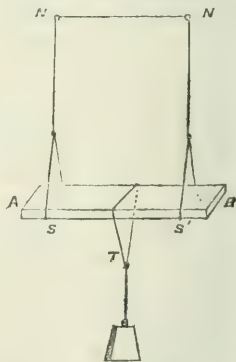
By Professor REUSCH of Tübingen.

To John Tyndall, Esq., F.R.S. &c.

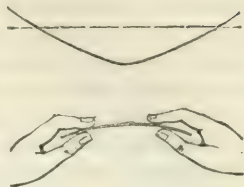
Tübingen, Jan. 28, 1864  
(Württemberg).

SIR,  
YOUR book has awakened my interest for that wonderful body ice; and I have devoted the recent cold weather to a more accurate determination of its coefficient of refraction for ordinary and for extraordinary light, as well as to the observation and measurement of the coloured rings of polarized light. In particular your interesting observation upon the layer of snow which sank away from the roof of the greenhouse and curved itself below (page 202), led me to make a direct experiment upon the deformation of ice—an experiment which answered perfectly, and a description of which will perhaps be more interesting to you than to anyone else.

A plate of clean ice, A B, about 100 millims. in length, 12 to 15 millims. in breadth, 3 to 4 millims. in thickness, prepared by sawing and subsequent rubbing with a warm metal plate, is laid in the loops S S of a cotton or silken string which pass above over two pegs, N, N'; a third loop, T, carries a weight of about 200 grms. The temperature of the circumambient space should not much exceed  $4^{\circ}$  to  $6^{\circ}$  C. After the lapse of 20 to 30 minutes a bending may be plainly seen, which is at first regular, but afterwards greater in the middle, in consequence of the middle string becoming imbedded in the ice, and of the diameter of the plate being diminished.



I have thus in different experiments, in which the distance  $SS'$  was 46 to 80 millims., observed bendings of 6 to 8 millims. Once, indeed, I was able to bend a thin lamella of ice between the fingers of both hands.



In one experiment I allowed the ice to break, and measured the dimensions of the surface of fracture as well as I could; the greatest tension and pressure in the outermost layers I found to be = 690 grms. to the square millimetre. It would not be without interest to determine the constants of elasticity and rigidity (*Festigkeit*) for ice. Putting  $2P$  for the greatest weight in the middle,  $2l$  for the length  $SS'$ ,  $b$  for the (horizontal) breadth,  $h$  for the (vertical) thickness of the plate of ice,  $A$  for the greatest tension in the outermost layer, we have

$$P = \frac{1}{b} A \frac{bh^2}{l}.$$

Our values are, for example,  $A = 690$  grms.;  $b = 10$  millims.;  $h = 3$  millims.;  $2l = 100$  millims.;  $P = 107$  grms.;  $2P = 214$  grms. If about 300 grms. be suspended from the middle, the plate would therefore soon bend, owing to the melting away consequent upon that degree of pressure. I believe that this experiment could be carried out in summer.

It has certainly happened to you, that in sawing through ice, the saw after a short time has ceased to act, the spaces between its teeth becoming filled up with freshly-formed ice, so that it passes along almost without friction. In truth the saw melts through the ice, the heat necessary for that being the equivalent of the work applied to the saw.

The finest plates of ice adapted for optical observations, and about 6 to 8 millims. in thickness, were found in my wash-hand bowl, a tolerably large tin vessel, in which the water (80 millims. in depth) froze during the very cold nights in my bedroom. (River- and spring-ice are always too little homogeneous for optical purposes.)

In dividing these precious plates of ice, I soon found that it was necessary to handle them like glass.

If the convex blade of a knife be passed over a piece of ice with a certain pressure, a sharp crack will result, and the plate may be broken in the direction of this crack, provided that the temperature of the ice



and of the air be below  $0^{\circ}$  C. Obviously the knife acts in this



instance like a diamond, which depresses minute particles of glass, and through the wedge-action of which a progressive linear cracking is produced which renders fracture possible. A mere scratch suffices neither for glass nor for ice. It is worthy of notice that these clear sharp cracks are possible in ice which is in contact with water in a warm room—a proof that ice preserves its brittle character at temperatures lying close to its melting-point. The breaking off at the crack is, on the other hand, no longer possible on account of the regelation in the upper part of the crack.

I remain,

Yours truly,

E. REUSCH,

*Professor of Natural Philosophy,  
Tübingen.*

#### XXXIV. *On the Dynamical Theory of Heat.*

By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.S.S.L. & E.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

TO state arguments in reply to the objections raised by Mr. Gill to the Dynamical Theory of Heat would be merely to repeat what has been published in various forms during the last fifteen years. It is possible, indeed, that new matter for discussion might arise from his statement that there is probably some “concealed source of error” in the experiments of M. Regnault on the specific heat of air, but not until Mr. Gill shall have specified the nature of such source of error and the probable amount of its effects; and then the option of answering him would rest with M. Regnault. My object, therefore, in writing this paper is not to argue against any of the opinions expressed by Mr. Gill, but to point out that the account which he gives of the dynamical theory of heat is incomplete and erroneous, through the omission to take any notice of one of its two fundamental principles,—the consequence being that propositions to which he objects on the supposition that they belong to that theory have no real connexion with it, or are directly opposed to it.

The fundamental principle which Mr. Gill has omitted to consider is that which has been called the “Second Law” of the mechanical action of heat. It is in some respects identical with that which, under the name of “Carnot’s Law,” formed a part of the Theory of Substantial Caloric; and may be treated either as an independent principle (as was first done by Professor Clausius in 1849), or as a consequence of the hypothesis that

thermometric heat consists in molecular motions (as was shown in a paper of mine of the same date).

For the purposes of the present paper, it is unnecessary to state the second law of the mechanical action of heat in its most abstract form. It will be sufficient to state the form which it takes when applied to a perfect thermo-dynamic engine: viz.—

*In a perfect thermo-dynamic engine, receiving heat at a higher absolute temperature and rejecting heat at a lower absolute temperature, the quantity of heat which disappears in the production of an equivalent quantity of mechanical work is, to the whole quantity of heat expended, as the difference of the two absolute temperatures is to the higher absolute temperature.* That ratio is the efficiency of the engine.

Mr. Gill erroneously represents as consequences of the dynamical theory of heat, two propositions which are contradictory of this law. The first of these is that, by the aid of a perfect regenerator, the whole of the heat rejected in a perfect thermo-dynamic air-engine might be transferred back to the air, so as to make its efficiency unity—in other words, to cause the whole heat expended to disappear in producing mechanical work. So far from this being a consequence of the dynamical theory of heat, it is in direct contradiction to the second law of that theory; according to which the efficiency of a thermo-dynamic engine cannot be increased beyond a certain function of the temperatures between which it works, by a regenerator or by any other contrivance whatsoever. In a paper published in the Philosophical Transactions for 1854, I have shown that, according to the dynamical theory of heat, the proper function of a regenerator is to save some of the heat which would otherwise be wasted in engines which do not fulfil the conditions of greatest economy required by the second law, and that, in an engine which does fulfil those conditions, the use of a regenerator is nugatory.

The second instance in which Mr. Gill, through not considering the second law, erroneously represents the consequences of the Dynamical Theory of Heat, is where he alleges that, “according to the dynamical theory, a common non-condensing steam-engine, working without expansion, utilizes only about  $\frac{1}{40}$  of the theoretical power of the steam. By the addition of condensation the efficiency is doubled, or the engine gives off  $\frac{1}{20}$  useful effect.”

No such consequences have ever been deduced from the dynamical theory of heat. In the numerous examples of the theoretical calculation of the efficiency of steam-engines which have been published since 1849 in the writings of various authors, it has been shown that the efficiency of an ordinary non-expansive

steam-engine, according to the dynamical theory of heat, ranges from  $\frac{1}{24}$ th to  $\frac{1}{15}$ th or thereabouts, and that by the aid of expansion that efficiency can be increased in ordinary cases to  $\frac{1}{10}$ th or  $\frac{1}{8}$ th, and in some special cases to  $\frac{1}{5}$ th.

There is one passage in Mr. Gill's letter which I trust that his own sense of justice will, on further consideration, induce him to withdraw. It is that in which he brings against M. Hirn the serious charge of having allowed himself to be "borne down by scientific authority" at the time of the change in his opinions as to the theory of heat—a change which M. Hirn has distinctly stated to have been the result of his own deliberate and independent judgment. Such a charge ought not to have been made in the absence of all proof, and on the sole ground of a difference of opinion\*.

I am, Gentlemen,

Your most obedient Servant,

W. J. MACQUORN RANKINE.

Glasgow, February 15, 1863.

XXXV. *On supposed Objections to the Dynamical Theory of Heat.*  
By Mr. JAMES CROLL.

*To John Tyndall, Esq., F.R.S. &c.*

SIR,

**I**N a paper by Mr. Gill, addressed to you, which appeared in the Philosophical Magazine of this month, he adduces certain facts which he considers cannot well be explained upon the principles of the modern dynamical theory of heat. Mr. Gill's conclusions appear, however, to have been drawn rather hastily.

Perhaps the following considerations will tend to remove his difficulties, in so far as his first two objections are concerned.

Let us suppose with this writer that 100 represents the quantity of heat required to double the absolute temperature of the cubic foot of air in his cylinder under constant volume. When

\* The following are the words in which M. Hirn sums up the results of his experiments on the steam-engine:—"Le résultat positif auquel je suis arrivé dans des recherches tout-à-fait récentes, c'est que quelle que soit la manière dont travaille une machine à vapeur; que ce soit avec détente ou non, avec vapeur saturée ou avec vapeur surchauffée, que ce soit avec de la vapeur à une pression peu différente de celle de la chaudière, ou avec une vapeur qui éprouve en passant de la chaudière au cylindre une chute de pression considérable, on trouve toujours qu'il y a une proportionnalité exacte entre le travail rendu par le moteur et la différence qui existe entre la quantité de chaleur fournie avant l'entrée dans le cylindre moteur et la quantité de chaleur qui reste à la vapeur à sa sortie du cylindre." (*Exposition analytique et expérimentale de la Théorie Mécanique de la Chaleur*, p. 84. Colmar, 1862.)



the piston under atmospheric pressure is allowed to rise while the absolute temperature is being doubled, 142 of heat are required. The additional 42 represents the amount of heat consumed in mechanical work in raising the load. When a quantity of heat equal to that which had been applied (142) is withdrawn from the air, the piston then falls to its original position; and in doing so, according to the received opinion among physicists, performs an amount of work equal to that expended in raising it. Mr. Gill disputes this point, and asserts that the piston in descending under the pressure of the atmosphere performs only one-fourth part of the above amount of work.

Now if the work that we are in search of were mechanical work, such as raising a weight or driving an engine, it is perfectly true that the descent of the piston will not perform as much work of this sort as is equivalent to the raising of the piston one foot under atmospheric pressure, because there are other effects to be produced by the descending piston besides mechanical work. First. The air has to be compressed into one half its present volume against its elasticity. Here we have work analogous to bending a spring, or winding up a watch. A portion of the *vis viva* of the descending weight is converted into the *potential* energy of elasticity. Secondly. The piston, in compressing the air, raises its temperature. Hence a portion of the *vis viva* of the descending piston becomes converted into that form of molecular motion called heat. Now if Mr. Gill will add the three effects together, he will find that their sum is exactly equal to 2116·4 foot-pounds, the amount of energy required to raise the piston when under the pressure of the atmosphere,—the air being, of course, assumed to be a perfect gas.

- |  |  |
|--|--|
| 1. Mechanical work performed by the descending piston, | } = 2116·4 foot-pounds,<br>the amount expended<br>in raising the piston. |
| 2. Work performed in compressing the air,              |  |
| 3. Work performed in heating the air,                  |  |

It is more than probable, however, for reasons into the consideration of which we need not at present enter, that the elasticity is entirely due to heat. But nevertheless this does not alter the fact that we must draw a distinction between the *vis viva* which is consumed in work, against that form of heat called elasticity, and that form called rise of temperature.

When the piston is at the top of the cylinder, each particle of the air, by virtue of the vibratory motion which it possesses in the form of heat, strikes against the bottom of the piston with a force proportional to its temperature. When the piston descends

and compresses the air into half its volume, the temperature being kept constant, each particle of the air still strikes the bottom of the piston with exactly the same energy as before; but the number of particles now striking the piston is doubled. Hence the elasticity of the air is also doubled. When the absolute temperature is doubled, for example, the elasticity is likewise doubled, because in this case each particle gives two strokes for one that it gave before.

Let us now refer to his next difficulty. Suppose a cylinder containing air under a pressure of two atmospheres is allowed to double its volume by rushing into another cylinder which had previously been made a vacuum. The particles of the air rushing into the vacuum with great velocity must strike against the sides of the cylinder and thus produce heat. But it is found, from the experiments of Dr Joule and others, that the air after expansion is at the same temperature as before it. This fact, he says, can only be explained upon the supposition that the specific heat of the air increases as it expands. But such a conclusion by no means follows. What is it that drives the particles of the air out of cylinder A into cylinder B? The elasticity of the air of course. Or, in other terms, the *vis viva* of the moving particles of the air as they enter the cylinder B, is derived from that form of molecular motion called heat possessed by the particles in the cylinder A. The particles in the cylinder A must then lose heat in consequence. The *vis viva* of the moving particles in the cylinder B becomes converted into the molecular motion of heat when the particles strike against the sides of the cylinder, and consequently the temperature must rise. But the rise of temperature in cylinder B is exactly counterbalanced by the corresponding fall of temperature in cylinder A. Hence the temperature of the air must remain unchanged. If the specific heat of the air increased with the diminution of density, as Mr. Gill supposes, then the temperature of the air after expansion ought to be less than before it, a conclusion contradicted by experiment.

I am, Sir,

Your most obedient Servant,

Glasgow, February 8, 1864.

JAMES CROLL.

[Out of deference to a practical experimenter, we gave Mr. Gill an opportunity of stating his objections to the Dynamical Theory of Heat. But we do not think that it would be desirable to prolong this discussion.—EDS.]

XXXVI. *On Indium.*

By F. REICH and TH. RICHTER of Freiberg in Saxony\*.

**A**LTHOUGH our investigations concerning the properties of this new element and its compounds may appear incomplete, owing to the extremely small quantities of this body which we have hitherto been able to obtain, we think it advisable to publish the facts we have already ascertained, as there is no immediate prospect of our obtaining an abundant source of the new metal.

The position of the brighter blue line seen in the spectroscope is found to be on division 98 of the scale, that of the weaker on 135, when the sodium line is on 38, and the blue strontium  $\delta$  on division 93. (If Na be placed on 50 and Sr  $\delta$  on 104, the In  $\alpha$  will be seen on 110, and In  $\beta$  on 147†.) We may here remark that if a suitable indium-salt be brought into the flame of a Bunsen's lamp, the flame becomes tinged a bright violet; so that the presence of indium can be ascertained without the aid of the spectroscope.

The fact of the reduction of the oxide with carbonate of soda on charcoal before the blowpipe has been confirmed. The metal forms a soft, ductile bead, which imparts streaks to paper on rubbing, and possesses a colour lighter than that of lead, being about the same as that of tin. The metallic bead dissolves in hydrochloric acid with evolution of hydrogen; and a platinum wire moistened with the solution exhibits in the spectroscope the blue line very strongly, although but for a few moments.

If the metal be heated by itself before the blowpipe, an easily fusible incrustation is obtained, which, when hot, possesses a dark yellow, but when cool a light straw-colour. This incrustation is volatilized with difficulty, and imparts the characteristic tint to the flame.

We have always obtained the ignited oxide of a yellowish colour, it has, however, invariably contained a trace of ferric oxide.

The hydrated oxide, as precipitated by ammonia, is white and gelatinous, adhering to the sides of the precipitating-glass. Tar-

\* Communicated by Professor Roscoe.

† I have examined the spectrum of a small specimen of oxide of indium kindly sent me by Professor Richter of Freiberg. For the purpose of obtaining a distinct spectrum, I place a small portion of the oxide on a platinum loop, moisten with hydrochloric acid, and observe the light emitted by a weak induction spark in passing over the chloride. In this way both the indium lines are distinctly seen; whereas if the bead be placed in the gas-flame only one line is seen, and that but faintly. When the sodium line is on division 50 of the scale of my instrument, and Sr  $\delta$  on 100.5, I find In  $\alpha$  on 107.5, and In  $\beta$  on 140.0.—H. E. R.



taric acid added to an acid solution of the oxide of indium prevents its precipitation by ammonia. If sulphide of ammonium be added to the tartaric acid solution saturated with ammonia, a voluminous white precipitate falls down, coloured slightly greenish from the presence of a trace of iron.

Potash precipitates the hydrated oxide from acid solutions in the same form as ammonia does, and as completely. Carbonate of sodium precipitates the indium compounds also completely, but the precipitate is crystalline. The white precipitate thrown down from boiling solutions, after drying, dissolves in dilute sulphuric acid with effervescence; it therefore consists of carbonate of indium.

The oxide ignited in a bulb-tube in an atmosphere of hydrogen underwent no alteration, and no water was formed. The oxide, mixed with powdered charcoal and ignited in a bulb-tube in an atmosphere of chlorine, yielded by moderate heat a very volatile chloride, which condensed in the cold part of the tube. This sublimate possessed a yellow colour, but it still contained a trace of iron, and a few crystalline scales appeared white and had a pearly lustre; so that it is probable that the yellow colour was due to a trace of ferric chloride.

The chloride of indium is extremely hygroscopic. If it is again heated, hydrochloric acid is evolved together with the chloride, proving that this salt is at least partially decomposed.

In the spectroscope the chloride exhibits the blue line with the greatest intensity; but owing to the volatility of this salt the line is only seen for a very short time. If, however, a small quantity of the oxide be brought into a small platinum spoon and moistened with hydrochloric acid, the blue line is seen on placing the spoon in the flame; and although the line does not appear quite so brightly as when obtained according to the former method, it remains visible for a much longer time; indeed, by moistening the spoon with fresh hydrochloric acid, the experiment may be repeated many times without more oxide being needed.

The hydrochloric acid solution of the reduced metal, treated with ammonia and sulphide of ammonium, gave a greyish-brown precipitate. Further investigations with larger quantities of the purified substance must, however, decide whether this is the true colour of sulphide of indium, or whether it is due to traces of impurities.

The mode of purification of the oxide of indium is readily understood from the above reactions. The original solution is treated first with sulphuretted hydrogen, the oxide of indium is then precipitated in the filtrate by ammonia, redissolved and

reprecipitated by potash, and thus separated from every metal except iron. By boiling the acid solution after saturation with acetate of soda the iron is precipitated, and the filtrate contains much of the indium, but a still larger quantity is carried down with the oxide of iron.

The best separation was effected by precipitating the iron carefully by bicarbonate of sodium. If there is a large excess of iron present, it is best to precipitate this metal at first incompletely, to filter, and then to complete the precipitation of the iron in the filtrate by a gradual addition of a very dilute solution of the bicarbonate. On boiling the filtrate and addition of a little more sodium-salt, the indium is precipitated as hydrate or carbonate.

The solution of the chloride of indium produced with ferrocyanide of potassium a white precipitate, tinged slightly blue by a trace of iron.

Ferriecyanide of potassium gives no precipitate. Sulphocyanide of potassium produces a pale red coloration due to a trace of iron.

As the oxide possesses many properties in common with alumina (from which, however, it is distinguished by its insolubility in potash, and its reducibility before the blowpipe), it is well to mention that when heated on charcoal and moistened with cobalt solution it does not yield a blue colour; and after heating, it completely although slowly dissolves in hydrochloric acid. By adding sulphate of potassium to a solution of sulphate of indium, no crystals of alum were obtained on evaporation.

We have also ascertained in what mineral indium occurs. By passing chlorine over the pure zincblende, arsenical pyrites, and iron pyrites contained in the mixed ore from which the indium had been prepared, it was found that the blende was the only one of the minerals which contained indium. The chloride of indium sublimes in the tube, and a portion of it is carried over into the wash-water, whereas the greater part of the chloride of zinc remains in the bulb.

We have likewise found indium in the distilled zinc prepared from the Freiberg blende, and we have obtained the oxide from this source. According to our experiments, a very large quantity of ammonia is required, in comparison to the yield of indium, in order to separate this from the excess of zinc, as we obtained only 1 part of the oxide of indium from 1000 parts of zinc dissolved. The precipitate thus obtained is dissolved, treated with sulphuretted hydrogen, after oxidation again precipitated with ammonia, and the indium and iron separated according to the method above described.

XXXVII. *Experiments on the Electrical Fly.* By CHARLES TOMLINSON, Lecturer on Physical Science, King's College School, London\*.

[With a Plate.]

EVERY lecturer on frictional electricity during the last hundred years must have made use of an elegant little toy known in England as the "electrical fly," and in French as the *tourniquet électrique*. The explanation of this instrument has changed its position several times on the shifting sands of electrical theory, and even in modern works of authority its action is differently explained. Thinking it to be my duty as a lecturer to satisfy myself on all points of scientific doctrine that might arise in my lectures, before I could hope to satisfy the more intelligent of my pupils, I have traced the history of the electrical fly and noted the various theoretical opinions respecting it†, and have also contrived some experiments with a view to settle the theory, at least in my own mind.

### 1. *History of the Electric Fly.*

The electrical fly was invented by Mr. Hamilton, Professor of Philosophy in the University of Dublin, and was first described by Mr. Wilson in 1760‡. He says, "Let a slender brass or iron wire, 5 or 6 inches long and finely pointed at each end, be fitted in the middle with a brass cap void of angles; then let half an inch at each extremity be bent in opposite directions till they are perpendicular to the rest of the wire, and in such a manner that, when the wire is suspended by means of its cap on a point of metal, it may be in a plane parallel to the horizon§. The pointed metal which supports this wire must be 2 or 3 inches long, and have its other end fixed into a small block of wood. Now if this block with a wire suspended be set upon an electrified body, the wire will turn round with a very great velocity, moving always in a direction contrary to that in which the electric fluid issues from its points, without having any conducting substance near it save that of the air; and if the wire be made to turn round by any force in the opposite direction, so that its points go foremost, it will, when electrified, soon be deprived of that motion and made to turn round the con-

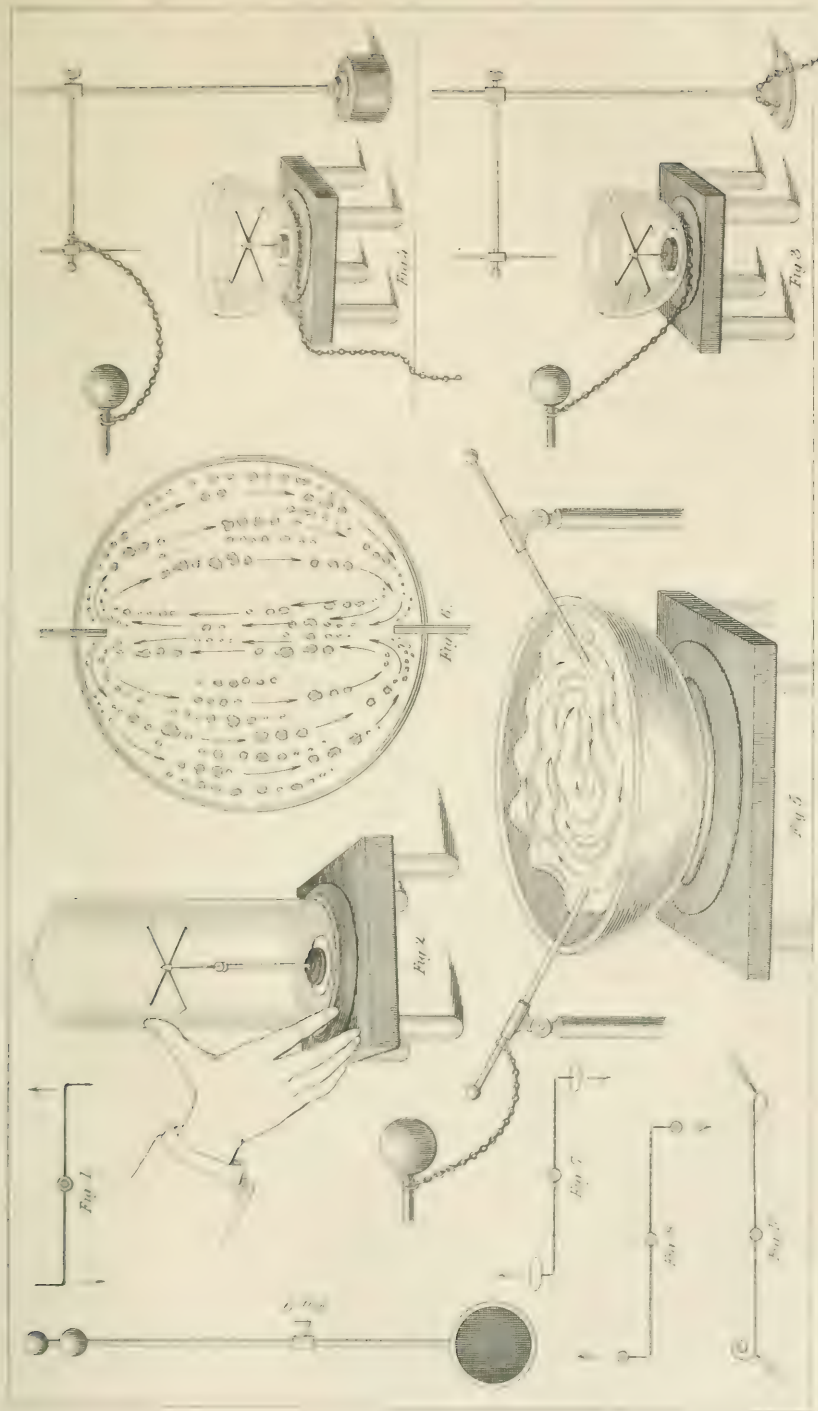
\* Communicated by the Author.

† In the following quotations from different authors, the passages in italics have been underlined by me.—C. T.

‡ Transactions of the Royal Society, 1760, "Further Experiments in Electricity," by Mr. Wilson, p. 905.

§ Some French writers represent the fly according to this description, as consisting of a single wire (see Plate II. fig. 1). English writers usually represent it as consisting of two wires crossed at right angles (fig. 2).







trary way. This experiment was contrived in order to try whether the electric fluid, which issues so freely from pointed bodies, would have any effect to move these bodies by its reaction; and that it has such an effect seems sufficiently manifest from the event. Mr. Hamilton apprehends that the electric particles, by their elastic force, issue directly forwards from the points, and endeavour to expand themselves, *but meeting with some resistance from the air*, force the wire to move backwards in a contrary direction—much in the same manner that a Catharine wheel is made to turn round in a direction contrary to that in which the small rockets attached to the periphery discharge themselves.”

Soon after this experiment was made known, Mr. Kinnersley\* repeated it with negative electricity, “expecting the needle to turn the contrary way, but was extremely disappointed, for it still went the same way as before.” He then endeavours to account for the fact in the following manner. He says, “When the stand was electrified +, I suppose that the natural quantity of electricity in the air being increased on one side by what issued from the points, the needle was attracted by the lesser quantity on the other side. When electrified negatively, I suppose that the natural quantity of electricity in the air was diminished near the points; in consequence whereof the equilibrium being destroyed, the needle was attracted by the greater quantity on the opposite side.”

Kinnersley attempts to justify this view by the following observations:—“The doctrine of repulsion in electrized bodies I begin to be somewhat doubtful of. I think all the phenomena on which it is founded may be well enough accounted for without it. Will not cork balls electrized negatively separate as far as when electrized positively? And may not their separation in both cases be accounted for upon the same principle, namely, the mutual attraction of the natural quantity in the air, and that which is denser or rarer in the cork ball?”

Dr. Priestley, commenting on these experiments, remarked that the pointed wire turning the same way, whether electrified plus or minus, may by some be taken as a proof “that the electric fluid issues out at the points in both cases alike, and by the reaction of the air is together with the points driven backwards—contrary to what ought to have been the case if the electric fluid had really issued out of the points in the one case and entered in the other”†.

\* Phil. Trans. 1762, p. 86, “New Experiments in Electricity, in a letter from Mr. Ebenezer Kinnersley to Benjamin Franklin, LL.D., F.R.S.” The letter is dated “Philadelphia, March 12, 1761.”

† History of Electricity, 4th ed. (1775) p. 395.



Beccaria, in his treatise\* published in 1771, represents the fly in the form of an Italian *S*, but makes two mistakes as to its inventor. He says (p. 331), "Points at the same time that they drive the air forwards are themselves driven back. Monsieur Jallambert [he means Jallabert] was the first who observed this repercussion of points, which the Abbé Nollet afterwards found to be uncertain."

Now it is quite true that M. Jallabert of Geneva was the first to observe the different effects produced by presenting a sharp and a round point to a charged conductor; and in 1748 he showed the experiment to the Abbé Nollet, who was not satisfied as to the constancy of the result; but neither in Jallabert's collected Essays† nor in any of Nollet's numerous works, have I been able to find any reference to the electric fly or *tourniquet*; and it is not to be imagined but that so eager an experimentalist and so popular a lecturer as Nollet would have made much of this elegant toy had he been acquainted with it. He performed a number of experiments on the action of points, and addressed one of his letters‡ to Franklin, stating Jallabert's experiments, his own, and his reasons for differing from some of Franklin's conclusions. Even in his later work§ I find no mention of the fly; but there is a good deal about points, and the engraving opposite page 378 represents the action of points in various forms of brush.

But to return to Beccaria. He says (page 338), "The celebrated question why points drive the air forwards, whatever the direction of the fluid, is reduced to the general fact that the electricity forces in contrary directions (or to opposite sides) the resistances through which it passes; because whether the electric fluid enter or escape, *it not only drives forward the air, but drives back the points also*" ||. He has just related the experiment of discharging a Leyden jar through a quire of paper, whereby a burr is produced on both sides; and from this and other experiments draws a general conclusion as to the expansive force of electricity.

A few years later Cavallo makes the motion of the fly to depend "upon the *repulsion existing between bodies possessed of the same electricity*; for whether the fly is electrified positively or negatively, the air opposite to the points of the wires (on

\* *Elettricismo Artificiale*. 4to. Torino, 1771.

† *Expériences sur l'Electricité*. Svo. Geneva, 1748.

‡ *Lettres sur l'Electricité*. 12mo. Paris, 1764-67. See vol. i. p. 124.

§ *Leçons de Physique*, vol. vi. (1771).

|| "Il fuoco elettrico spinge in parti contrarie le resistenze, per le quali tragitta; perciocchè o che il fuoco elettrico entri, ovvero che esca, non solo spinge il venticello, ma retrospinge anche le punte."

account of the points easily transmitting electricity) acquireth a strong electricity analogous to that of the points; and therefore *the air and the points must repel each other.*" This explanation, he adds, "is confirmed by observing that the above fly not only does not move *in vacuo*, but even if placed under a close receiver it will turn but for a little while and then stop; for the quantity of air contained in the receiver may become readily and equally electrified"\*. Cavallo states in a note, that when the finger is placed outside the glass opposite to one of the points of the fly, they will move again briskly, and by altering the position of the finger round the glass its action may be continued till most of that part of the glass is charged. He explains the action of the finger in withdrawing from the outside of the glass a portion of its natural electricity, whereby "it receives the fluid of the electrified air on its inside surface; hence this air is put in a state of being again electrified by the point of the fly, which renews the motion."

Cuthbertson says† that the motion of the fly "is occasioned by the action of the electric fluid against the electrified air near the point."

Biot‡, adopting the theory of Poisson, that electricity is held to the surface by atmospheric pressure, states that the motion of the fly could hardly be produced in an absolute vacuum, because the electricity of the fly would be instantly dissipated; but that the motion can readily be produced in free air *if the points be sufficiently sharp to enable the electricity to accumulate at their extremities so as to overcome the atmospheric pressure.* The motion is then produced by reaction precisely in the same manner as if the wire were a hollow tube, bent at the ends in opposite directions, from which water or mercury were flowing. In such case the pressure on the orifices being reduced to zero, while that on the tube opposite the orifice is still acting, motion is produced in a direction opposite to that of the issuing fluid, and the wire thus turns on its centre.

The writer of the article "Electricity" in the *Encyclopædia Metropolitana*, published about 1824, says (p. 78), "Each of these points will give off a current of electricity, which from the *reaction of the resisting medium (the air)*, will cause the system of points to revolve backwards with considerable rapidity."

Roget§ thus explains the action of the fly:—"Each of the

\* A Complete Treatise on Electricity, third edit. (1786) vol. i. p. 296.

† Practical Electricity (1807), p. 24.

‡ *Traité de Physique* (1816), vol. ii. p. 324.

§ Library of Useful Knowledge. Natural Philosophy, vol. ii. (1832), "Electricity," p. 26. In my copy of this work the engraving of the fly with its supporting stem is placed upside down.

points will give off a stream of electricity; this will remove a part of the pressure which the fluid would have exerted on that side if no efflux had taken place; but as the pressure of the fluid on the opposite side of the wire in the opposite direction still operates in full force, the wire will be impelled in the direction of that force, that is, in a direction opposite to that of the stream; and this taking place at all the four points, the whole system will revolve backward with considerable rapidity."

Becquerel\* says, "The experiment of the electric fly shows that the motion of bodies in electrical phenomena is really due to the *difference in pressure* which the air exerts on all the points of these bodies." Then, after describing the tourniquet, he adds, "The electricity escaping by the points, *the pressure exerted by the air* on the extremities being greatly diminished, the needle turns in a contrary direction."

Despretz†, after describing the fly, says, "*Ce phénomène est absolument pareil à celui que nous avons observé pour les liquides.*"

Lardner‡ says, "When the electricity comes from the conductor to the wires, it will escape from the wires at these four points respectively, and the force with which it leaves them will be attended *with a proportionate recoil*, which will cause the wire to spin rapidly on the centre."

Harris§, speaking of the electrical aura or gale, says, "The reactive force of this current upon the point itself, and from which it appears to flow, is so great as to give the point motion in a reverse direction when free to move."

Riess|| says, "This rotation arises from the *mutual repulsion of the electrified points and the particles of air*"¶.

De la Rive\*\*, in 1853, though not altogether satisfied with it, adopts the French theory as given by Biot in 1816. He says, "In this experiment the electric fluid that is spread over the stems of the mill exercises everywhere *a pressure on the surrounding air*; if it found no escape, the opposite pressures being always equal, the apparatus would remain at rest. But it escapes by the points *where it overcomes the atmospheric pressure*; and as it no longer exercises any pressure on the orifice of escape, the pressure which continues to be exercised on the opposite point

\* *Traité expérimentale de l'Electricité*, vol. ii. (1834) p. 194.

† *Traité de Physique*, fifth edit. Bruxelles, 1837, p. 271.

‡ *Manual of Electricity*, &c. Cabinet Cyclopædia (1841), vol. i. p. 330. See also the *Handbook of Natural Philosophy*, "Electricity," &c. (1856) p. 62.

§ *Rudimentary Electricity*, second edit. (1851), p. 151.

|| *Die Lehre von der Reibungselectricität* (Berlin, 1853), vol. ii. p. 153.

¶ "Diese Rotation entsteht durch gegenseitige Abstossung der elektrisirten Spitzen und Lufttheilchen."

\*\* *Treatise on Electricity*. Translated by Walker (1853), vol. i. p. 130.



causes the movement by a *true recoil, analogous to that produced by liquids or gases upon reacting machines during their escape.*"

Pouillet\* refers the motion to a *recoil* exactly similar to that produced in the *hydraulic tourniquet*.

Ganot† denies the truth of this statement, and falls back upon the explanation given by the electricians of the last century. "This motion," he says, "is not an effect of reaction comparable to that of the hydraulic tourniquet, as many physicists suppose, but it is an *effect of repulsion between the electricity of the points and that which they communicate to the air*. The electric fluid, accumulating towards the point, escapes into the air, which thus becomes charged with the same electricity as that of the points; *it thus repels the points, and is at the same time repelled by them*. It is admitted that the tourniquet does not rotate in vacuum; and if we approach the hand near it whilst it is turning in air, a slight wind will be felt, occasioned by the displacement of the electrified air."

Eisenlohr‡ also refers the motion to the repulsion *occasioned by the electrization of similar particles*.

In the foregoing statements we have no less than five theories to explain the action of the electrical fly. *First*, there is what may be called the *recoil* theory, advocated by Hamilton, Priestley, the *Encyclopædia Metropolitana*, Despretz, Lardner, Pouillet, and Harris. *Secondly*, the *attraction* theory, advocated by Kinnersley. *Thirdly*, the *expansion* theory, advocated by Beccaria. *Fourthly*, the *repulsion* theory, advanced by Cavallo, Cuthbertson, Riess, Ganot, and Eisenlohr. *Fifthly*, the *atmospheric pressure* theory of Biot, Roget, Becquerel, and De la Rive.

Among these five sets of theories, the repulsion theory seems to me best adapted to represent the truth; but there are so many interesting points of electrical science involved in the behaviour of the fly under various circumstances, that it seems difficult to represent its action in a few words. There is, first, the behaviour of the fly in air; secondly, when wholly or partially enclosed in vessels of glass or of metal; thirdly, when placed in rarefied air of different pressures; fourthly, when poised in liquid dielectrics; and fifthly, when the points are concealed or otherwise modified.

In any theory of the fly, it seemed necessary from the beginning of the inquiry to understand clearly the action of points; and desiring no better guide than Dr. Faraday§ in this matter,

\* *Elémens de Physique*, seventh edit. (1856) vol. i. p. 508.

† *Traité de Physique*, seventh edit. (1857) p. 566.

‡ *Lehrbuch der Physik* (1860), p. 504.

§ *Experimental Researches in Electricity*, vol. i. second edit. 1849. See the whole of the instructive section entitled "The Electrical Brush," commencing p. 454.

the action, if I read him correctly, appeared to be this:—When a metal point projects from an insulated charged conductor, the particles of air at and about the point become more highly polarized than at the parts of the conductor where the lines of inductive force cannot so readily accumulate. The particle of air next the last particle of metal on the point thus becomes so highly charged that it soon discharges itself on the point, becomes similarly electrified to the point, is repelled and acts inductively upon surrounding particles to some distance. Meanwhile a second particle, taking the place of the first, becomes so highly charged as, in its turn, to discharge itself upon the point, and becoming similarly electrified, is repelled. In this way a rapid interchange of particles of air is kept up, producing the *convective* or *brush discharge*; and so rapid is the interchange of particles, that the effect is that of a strong wind sufficient by some mode or other to give motion to the point, if free to move. In the case of the horizontal electrical fly, there are four points all briskly engaged in this work of convective discharge, so that the motion of rotation in a direction contrary to that of the points is exceedingly rapid.

In the following experiments (unless otherwise stated) a fly of brass wire  $2\frac{3}{4}$  inches in diameter was used; it weighed 33 grains; it had four arms, and was poised by its central conical cap upon a steel point 5 inches high rising from a brass support. When placed on the prime conductor, a short steel needle was used as the supporting point. The electrical machine had a plate 2 feet in diameter\*.

## 2. *The Fly in air.*

When the fly on its stand is placed on the table at different distances below and away from the prime conductor without any connexion therewith except through the medium of the dielectric air, and also when held 12 or 15 inches above the prime conductor, it rotates briskly. If held in the aura from a blunt point the fly becomes more active, and also rotates some feet above or beyond the aura proceeding from a sharp point.

In all these cases the fly is uninsulated. If the operator stand on an electric stool and hold the fly near the prime conductor or in the aura, or if the fly be placed on an insulating stand, there is no motion whatever, because in all these cases the operator and the fly are in a polar condition like the air, and, like it, also

\* I occasionally used other flies; one with only one arm, a small ivory ball on the other side of the cap being used as a counterbalance. Also a fly with two arms. This and the former oscillated too much. I had two flies with four arms, one with five, and one with eight. The last moved slowly, but was too heavy for most purposes.

seeking to discharge upon the prime conductor, but not being able to do so, there can be no motion of the fly\*.

### 3. *The Fly enclosed.*

When the insulated fly is covered with a bell-glass and electrified, there can be no continuous motion, because the particles of air cannot discharge themselves and escape to make room for a continuous succession of uncharged particles from a distance. Cavallo's experiment was repeated in the following form:—A copper disc 6 inches in diameter was placed on an insulating stand, and the fly on the centre of the disc (see fig. 2). The fly was now covered with a clean dry bell-glass receiver 12 inches high and  $4\frac{1}{2}$  inches in diameter; and a chain was carried from the prime conductor into contact with the copper disc. On working the machine, the fly rotated a few times and then stopped. On touching the stand there was no motion of the fly. On touching the copper disc, the fly made a few turns and then stopped. On presenting the finger to the outside of the glass on a level with the fly at the distance of 8 or 10 inches, the fly immediately started off at a rapid pace, and continued to rotate so long as the finger was held. When the motion of the fly ceased, a sharp metal point was presented at the distance of 27 inches, and the fly started off again.

Cavallo explains this effect with considerable sagacity. The mistake made by him, in common with some more modern writers, seems to be in describing the electricity as passing off so readily from a point and so electrifying the air; whereas it is the air that fetches the electricity from the point and so dissipates the charge. In the experiment just narrated the electricity is conveyed into the bell-glass by the fly, and its metal support and the confined portion of air are made polar, but cannot discharge upon the points because, being confined and insulated, there can be no circulation. For a similar reason, it cannot get rid of its surplus electricity by charging the bell-glass and returning to the points for a fresh charge. Directly, however, a conducting point is presented to the outside of the bell-glass, a portion of the vitreous electricity is drawn off from the outside, the inside becomes capable of receiving a charge, and the particles of air

\* I need scarcely in this place answer the question which has been frequently put to me in the course of these experiments, why the fly insulated on the prime conductor rotates, when if insulated apart from, but near to, the prime conductor it does not rotate. It is obvious that on the prime conductor the particles of electrified air can discharge themselves upon the points of the fly, while insulated away from the prime conductor it forms part of the dielectric medium; uninsulated away from the prime conductor, the charged particles of air can discharge themselves upon the points of the fly and thus produce motion.



travel backwards and forwards between the points of the fly and the inner surface of the bell-glass. Brushes are given off by the fly, which now rotates, and a star is seen on the metal point held towards the outside of the bell-glass.

A charge was given to the bell-glass by holding it in both hands and allowing the inside to rub against a chain hanging from the prime conductor. On lowering the receiver over the insulated fly, it moved slowly for a short time, but on touching the copper disc and also the side of the glass opposite the fly, the motion became very brisk. In this way the fly has been started no less than sixteen times, after having been brought to rest each time (see fig. 2).

In this case the bell-glass is converted into a Leyden jar: the inside is vitreous, together with the copper disc and the enclosed fly and metal support. The outside of the bell-glass is resinous. On touching the copper disc alone, there is no motion of the fly any more than touching the outside of the glass alone, because in either case the balance between the outer and inner charge is not disturbed. If, however, the copper disc and the outside of the bell-glass be both touched, a portion of the charge is liberated, induction can go on between the included air and the points, and the fly is started by the action of the discharging particles. Soon, however, electrical equilibrium is re-established, the momentum of the fly is expended, and the fly is brought to rest. Touching the copper disc and the outside of the bell-glass neutralizes another portion of electricity; the enclosed air is again in a condition to relieve its tension by discharging on the points of the fly, to which motion is thus again imparted. Of course the larger the portion of the outside that is touched the stronger is the action on the fly, and the longer does it continue to spin.

This experiment is well adapted to the lecture-table, and produces surprise by the facility with which the fly is started apparently by a very inadequate operation. It also illustrates the action of the Leyden jar, and how well the coatings may be dispensed with. That the copper disc and fly are in the same state as the inside of the jar was proved by means of the gold-leaf electrometer. The experiment was also tried with resinous electricity on the inside with similar results.

The facility with which a coated Leyden jar may be discharged by presenting a point to the knob is well known. If that point were free to move, it would first be attracted, and then repelled in the act of doing so\*. The fly behaves in a similar manner.

\* This effect may be shown by suspending a darning-needle by tying a filament of silk to the centre of its length; or if required to be uninsulated, passing the needle through one end of a very narrow strip of tinfoil and

If brought near the knob of a charged jar, the points are first attracted and then repelled, after which the fly rotates briskly, and continues to do so until the charge is nearly drawn off. The experiment succeeds equally well whether the jar be charged vitreously or resinously.

The fly was placed within a cylindrical spangled Leyden jar, 10 inches high and  $4\frac{1}{2}$  inches in diameter; the tinfoil coatings rose to the height of  $8\frac{1}{4}$  inches; a chain from the prime conductor trailed down the inside of the jar. The fly was arranged so as to be on a level with the top of the coatings. Soon after the electrical machine was set in action the fly began to rotate and dissipated the charge, so that it was impossible to overcharge or even to fully charge the jar. When the electrical machine was stopped, the fly continued to move until only a weak charge was left in the jar.

A second fly poised on a point in the hand was set spinning when held in the powerful electrical aura which diverged from the mouth of the jar, and which could thus be traced to a great distance. The aura was sufficiently powerful to give motion to a heavy fly 6 inches in diameter and weighing 123 grains, so long as it was uninsulated; but directly it was insulated it ceased to move, for the reason already given.

The fly was lowered in the spangled Leyden jar so as to be about an inch below the top edge of the coatings. On charging the jar, the fly did not rotate until induction was assisted by holding a finger just over the jar. On raising the fly so as to be on a level with the mouth of the jar, or even higher, motion became much more sensitive, on account of the facility with which the polarized particles of air could discharge on its points.

When the fly was placed on a point within an insulated copper saucer, 5 inches in diameter and  $1\frac{1}{2}$  inch deep in the middle, and the electricity sent in by a chain attached to the lower end of the point, there was no motion whatever, even though the finger or a metal point were held over the fly, the whole of the electricity passing to the outside of the copper dish.

#### 4. *The Fly in rarefied air.*

A small air-pump was insulated and the fly placed on its table and covered with a bell-glass receiver. The air was exhausted to 1 inch of the gauge, and the apparatus electrified; no motion whatever could be produced in the fly either by presenting the finger or a metal point to the receiver, or by occasionally

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hanging it up by the other. In either case the needle is alternately attracted and repelled if brought within a few inches of the knob of a charged jar.

uninsulating the apparatus by touching the air-pump table. When the room was darkened, brushes were seen from the four points of the fly.

Here I imagine the dielectric particles were not in sufficient number to produce mechanical action by their discharge upon the points. When the gauge stood at 2 inches, the fly spun round with great rapidity on presenting the finger to the receiver; and its sensibility went on increasing as more air was admitted, so that at  $5\frac{1}{2}$  inches' pressure the fly obeyed the action of a metal point held 14 inches and upwards from the receiver. When the mercury stood at 8 inches, the fly was very sensitive and revolved rapidly with a librating motion towards the finger. If the operator stood on an electrical stool and presented a finger or metal point to the receiver, there was no motion even when it was most sensitive.

Here the glass became converted into a Leyden jar, the inside highly charged with vitreous electricity, and the outside with resinous, brushes of which were thrown off freely on presenting the finger, which served as a vent to the outside; while the air on the inside, by constantly discharging on the points, thus got rid of the surplus vitreous charge as fast as it was poured in.

### 5. *The Fly in liquid dielectrics.*

Faraday found\* that when a dielectric, such as turpentine, was electrified in a glass vessel by means of two wires dipping into it in different places, "one leading to the electrical machine and the other to the discharging train, on working the machine the fluid will be thrown into violent motion throughout its whole mass, whilst at the same time it will rise two, three, or four inches up the machine wire and dart off in jets from it into the air."

In the museum of this College is an apparatus fitted up by my friend the late Professor Daniell for showing these effects. It consists of a finger-basin with a wire passing through the bottom for the purpose of mounting on the prime conductor. Turpentine containing particles of gold-leaf was poured into the vessel, and on working the electrical machine and holding a metal knob over the vessel, the currents in the dielectric were made visible by means of the gold-leaf particles. I tried to mount the fly in this apparatus, but not succeeding to my mind, I drilled a hole in the bottom of a finger-glass, passed a cork into the hole, and ran a darning-needle through the cork so as to project into the vessel more or less as required. The glass was mounted on an insulating stand, and a chain from the exterior portion of the needle led to the prime conductor; but still further to insure

\* Experimental Researches in Electricity, § 1595.



metallic contact, a disc of tinfoil was attached to the bottom of the vessel (see fig. 3). The fly moved very easily on its point within the vessel.

On pouring in enough turpentine to cover the fly completely, and holding an uninsulated point over the vessel, the liquid was thrown into great commotion, welling up the sides of the vessel and spitting out numerous globules. But the most remarkable effect was on the fly: it rotated rapidly and uneasily, frequently rising and falling on its support; but its motion was not, as in air, with the points backwards; the points were now moving forwards. I next connected the chain with the point over the vessel, insulating it on a cake of resin, and connected the finger-glass with the earth (see fig. 4). There was still much mechanical disturbance, but the fly constantly rotated with the points forwards.

When benzole was substituted for turpentine, there was a greater mechanical disturbance than before, but the fly rotated with the points forwards. On trying the action of a knob instead of a point over the liquid, a spark passed and kindled it. The flame was immediately extinguished by placing a copper disc over the vessel, when, on working the machine and holding a point over the vessel as before, the fly rotated with the points backwards as in air.

In paraffin oil the fly rotated at first with the points backwards as in air. On distilling the oil and repeating the experiment, the fly rotated with the points forwards. I obtained this result with several kinds of paraffin oil; and on purifying them, and two or three specimens of turpentine by distillation, it seemed as if the fly rotated in one direction in the crude oil, and in another direction in the distillate. By multiplying the experiments from day to day, contradictions crept in which showed that I was not on the right track. It then occurred to me that the finger-glass, though  $4\frac{1}{2}$  inches in diameter, did not present a sufficient surface of liquid to enable me to study the effects that were going on in this miniature *mælstrom*, where the confusion was so great that nothing could be distinctly seen, and the mechanical action was so violent as often to throw the fly from off its support.

I therefore proceeded to examine the action of electricity on a liquid dielectric contained in a shallow glass dish, 6 inches in internal diameter, and less than an inch in depth in the centre. Paraffin oil was poured into this, and the wires of a universal discharger were made to dip into it at opposite points near the rim, one wire being connected with the prime conductor, and the other passing to the earth. The moment the electrical machine was worked, a wave started from each wire, showing

the simultaneous action of the two opposing forces, and meeting in the centre broke into a whirlpool of a sufficiently definite character to produce rotations (see fig. 5). A cork thrown on the surface was whirled round in circular and elliptical lines from wire to wire, showing clearly that the action was one of attraction and repulsion. The direction of the curve was from right to left; but I found that on shifting the places of the wires with respect to each other, the direction of the curve could be varied from right to left or from left to right at pleasure. Here, then, was an explanation of the contradictory effects obtained with the fly in liquid dielectrics. The fly had no more to do with the generation of the current than the cork had; it was merely swept round in the current, in whatever direction that might be, according to the line of least resistance connecting the two wires. A hole was now drilled through this shallow dish for the reception of a needle-point on which the fly was mounted, when it was found perfectly easy to make the fly rotate in either direction by a slight adjustment of the conducting wires.

In this experiment with the cork playing between two wires on the surface of the liquid dielectric, we have only a horizontal adaptation of Volta's experiment with the pith-balls between oppositely electrified surfaces, or the dancing figures between two electrified copper discs, or Franklin's electrical pendulum.

In this form of the experiment a number of beautiful electrical effects were obtained which do not belong to my present subject; but there are one or two that may be mentioned as throwing light upon the action of points on a dielectric medium. A number of particles of gold- and silver-leaf were put into turpentine contained in the shallow dish, and on working the machine they simply arranged themselves between the two wires as a conducting link\*; but on removing the earth-wire and holding a point over the liquid, induction took place, the particles became polarized, the metallic particles started asunder, and a current set in in the most beautiful and orderly manner, the metallic particles flowing in several parallel lines towards the prime conductor wire, striking upon it, and immediately being repelled, they passed away on either side of it round the edge of the liquid in symmetrical curves up to the negative point held over the vessel, and then set in as before toward the positive wire (see fig. 6). When the latter was raised a little way out of the liquid, and the negative point inserted into it, a rapid series of discharges took place among the particles, lighting up the vessel, and producing a beautiful effect in the dark.

\* Faraday obtained a similar result with particles of silk between two conducting wires in turpentine. See *Experimental Researches*, § 1350.

When the wires of the universal discharger were not more than 2 inches apart in turpentine, the effects were very energetic. A point held over the vessel at the distance of some feet produced a regular upward rain of turpentine; and even when the machine was turned slowly, the turpentine played and discharged upon the point as we must suppose air to do. A knob held over the vessel produced a complete waterspout; and many seconds after the machine had ceased to be worked the turpentine retained its charge, as was evident by the pit produced in the liquid when a point was held over it at the distance of some inches.

### 6. *Modification of the points.*

Seeing then that it is easy to make the fly rotate in liquid dielectrics either with the points backwards as in air, or with the points forwards, and not being able to trace any essential difference between air and a liquid dielectric except in density, I saw no reason why the fly should not be made to rotate in air with the points forwards. But this was no easy matter so long as the points existed in their normal condition. By attaching little discs of tinfoil to them by means of sealing-wax (fig. 7), the fly rotated with the points forwards without any difficulty, or simply by attaching knobs of sealing-wax to the points (fig. 8) they rotated forwards\*; but what was very curious, if a candle were within a foot or two of the fly with the wax knobs, it rotated backwards; or if, while rotating forwards, the flame of a spirit-lamp were brought near, it soon came to rest and resumed its rotations backwards. I next altered the fly so as to get rid of the bend in the arm, and found that straight arms with discs of tinfoil at the end rotated very well. Several forms of fly were constructed on this pattern; but the best form was a disc of tinfoil within a key-ring attached to a knitting-needle 9 inches long passing through a cap, and balanced with a brass knob at the other end, as in fig. 9. One side of the disc was coated with sealing-wax, and it rotated well with the metal side forwards; and if set going the other way, it soon pulled up and rotated with the metal side forwards.

These experiments seemed to favour Mr. Kinnersley's theory of attraction, and I worked for some time in the direction pointed out by it. The question was, at what part of the fly, on the attraction theory, was the pull applied? and, on this theory, what was the influence of the point? Could the point be got rid of in any other way than by covering it with discs, or concealing it with wax? It could be bent inwards so as to form a loop (fig. 10). This was accordingly done, and to make the direc-

\* I afterwards found pellets of bees' wax much more convenient.



tion of the force visible, I filled the loop with thick gum-water and saw it dragged out in the direction of the brush (fig. 10). I next fixed the fly on its point with a little bees' wax, and noticed its behaviour in the dark, when a noisy little brush was seen from each of the shoulders (fig. 10); the effect of doubling in the points being to transfer the brush from the sharp point to the nearest approach to a point that the fly now presented, namely the kind of rough blunt point formed by the shoulder. And even here a selection was made: the brush proceeded from that part of the shoulder which was not so nicely rounded as the other part, but where an obtuse angle had been formed by the pliers used in bending-in the point. Similarly in the other experiments where the point had been tampered with, a brush could always be traced from the shoulder (figs. 7 and 8) and from the edge (fig. 9). In order to get rid of these brushes, I had the balanced disc, fig. 9, nicely constructed by an instrument-maker. The disc was turned into the shape of a well-rounded ring of wood, which was filled up with shell-lac, and gilt on one side. A screw at the end of the wire entered the wooden rim, and at the other end of the wire was a brass balance-knob, while at the extreme end was a pith-ball; in this way all points were avoided. When nicely poised on its point on the prime conductor it moved wildly to and fro, as if bewildered by the multitude of objects that attracted it on all sides, but there was no rotation. When the finger was approached towards it, it swung round and acquired a momentum which made it revolve for some turns; but it soon came to rest, and would revolve either way according to the position of an uninsulated conductor held towards it at the distance of many inches, or even some feet.

With respect to the action of flame in reversing the motion of the fly with wax knobs, that seems to be simply a case of repulsion. Flame and hot air near the prime conductor convey away the same electricity as that of the prime conductor\*; so that if, while the fly with wax knobs is rotating with the points forwards, a flame be brought near, we bring up a highly mobile insulated conductor charged +, which acts by repulsion upon the wax knobs as being the largest and most prominent objects on the fly. By this repulsion the fly reverses its course, the brush from

\* The action of a flame, such as that of a spirit-lamp, seems to be a double kind of convection, calorific and electrical. The lines of hot conducting air moving from the flame become electrified in the same way as the prime conductor, and extend some feet from the machine. I have been surprised at the distances at which blasts of very hot dry air have swept across my face, producing an uncomfortable sensation, while endeavouring to trace them by means of a light fly poised on a fine point. That these conducting masses of dry air were in the same state as the prime conductor was proved by means of a gold-leaf electrometer.

the shoulder ceases to be heard, and the action is now between the + flame and the + knobs. A point held near the fly has a similar action to that of the flame, and also conducting vapours such as ether, &c.

The conducting action of flame is well shown in the following experiment. A metal-pointed stand, connected by a chain with the prime conductor, was placed in the centre of a dessert plate in which were put some coils of lamp-cotton wetted with spirits of wine. The fly on the point spun with great rapidity when the machine was worked; but when the spirit was kindled the fly stopped, and did not begin to spin again until the flame had died out.

### 7. Conclusion.

A cork playing on the surface of a liquid dielectric between two oppositely electrified wires, I have already compared with the dancing figures and pith-balls, and the electric pendulum. In the electric pendulum, a pith-ball or other light object is suspended between the knobs of two Leyden jars differently electrified. The ball is attracted by one knob, and is similarly electrified and then repelled; or what is the same thing, it is attracted by the other knob, by which it is discharged, similarly electrified, and then repelled, and in this way a rapid action is kept up, the ball describing various kinds of curved figures between and around the two knobs, sometimes striking them with a smart blow such as can be heard all over the room—at other times not touching them, but parting with its charge to the electrified particles of air which, like itself, we must suppose are being similarly attracted and repelled by the two excited knobs. That is, the particles of air as they leave either knob being charged, say +, encounter the pith-ball electrified, say —, and combine with its electricity in proportional quantities to neutralize it and themselves; but as there are myriads of particles leaving the + knob, they take the place of the neutralized particles, and communicate a plus charge to the pith-ball before it has time to reach the brass knob, so that it is actually in a condition to be repelled by the + knob and attracted by the — knob without touching either. The rapid whirling motion of the pith-ball in air and of the cork in paraffin oil, &c., must, I suppose, represent what takes place among the particles of air and the particles of the liquid dielectric; but there is this difference between the two media. Introduce an uninsulated point, free to move, into polarized air, and convective discharge sets in upon and in the direction of that point; the particles become similarly electrified to the point, and repulsion takes place with sufficient force to cause the point to move backwards. In a

liquid dielectric the currents produced depend for their force upon the amount of electricity sent in and its tension, and for their direction upon the position of the two conducting wires, the one that brings the charge and the other that conveys it away. The density of the medium will render the momentum of the particles so much greater in turpentine than in air in the proportion of their respective specific gravities. Hence a point, or a system of points free to move in the liquid dielectric, will not so much determine the direction of the current as be itself determined by that direction. It is in fact helpless in so dense a medium. It is swept round by the current, and moves points forwards or points backwards with the current.

If this be the true state of the case (and my experiments lead me to no other conclusion), the theory of the fly requires a different expression for an aerial, as compared with a liquid dielectric; its behaviour is also different in air of different densities, and also when wholly or partially enclosed,—also when the points are covered; and even then there is a different action in the presence of flame. In short, there is no one expression that fairly represents the electric fly. It modifies its behaviour according to circumstances; and, like a good subject, has no law of its own, but conforms to the laws of the community of which it is a member.

King's College, London,  
January 1864.

XXXVIII. *On Parathionic Acid.* By AUGUSTUS BEAUCHAMP NORTHCOTE, B.A., F.C.S., *Lecturer on Chemistry in Exeter College, Oxford* \*.

SINCE the days of Sertürner†, who in 1819 announced that in the residues from the preparation of ether he had discovered three acids related to each other, and composed of alcohol and sulphuric acid, the opinion that there exist certain modifications of the sulphovinic acids has been gaining ground, until of late years it has become a matter of certainty. Placing isethionic acid apart, the experiments of Gerhardt and Berthelot on the sulphethylic compounds, and of Church on the sulphomethylic, have shown that side by side with the sulphovinic acids, and produced by processes which should have yielded the ordinary varieties of those acids, there stand compounds possessing sufficiently well-marked characteristics to allow of their ranking as isomeric modifications.

The whole question of isomerism has recently assumed pecu-

\* Communicated by the Author.

† Gmelin (Cav. Soc.), vol. viii. p. 415.



liar interest from the researches of Wurtz, Kekulé, Boutlerow, and others; and the difference of molecular grouping has been clearly manifested, although much light has not yet been thrown on its cause or even its manner. Great part of the work thus accomplished lies within the limits of the alcohol series; and the production by Wurtz, Boutlerow, and Friedel respectively of isomers of the amyle and propyle alcohols lends a powerful aid to the observations which have been made in the same series with regard to the sulphovinic acids.

Hennel\* in 1826 proved that sulphethylic acid was produced when olefiant gas was absorbed by oil of vitriol; and Berthelot in 1855 found that when the sulphethylic acid obtained by this means was saturated by carbonate of barium, ordinary sulphethylate of barium was not the product, but that far more stable isomeric barium salt, the acid of which Gerhardt had already shown to result from the decomposition of the ordinary sulphethylate of barium by the boiling of its aqueous solution, and to which he had applied the name of Parathionic acid.

A parallel instance to this isomer in the sulphethylic series has been found by Church† in the sulphomethylic. He has shown that in the decomposition of sulphomethylate of barium by water an acid is obtained, the barium salt of which is stable at the temperature of its boiling aqueous solution, bearing thus the same relation to ordinary sulphomethylate of barium as Gerhardt's parathionate does to the sulphethylate of barium. This result he attributes to the intermediate production of sulphate of methyle, decomposable by water, supporting his conclusion upon an observation made on the decomposing influence exerted by water upon the sulphate of ethyle-methyle, in which change both the alcohols and both the modified acids are produced.

The conditions under which I have encountered what I can have but little doubt is the same isomer are different from these. I had prepared a solution of potash in pure absolute alcohol at least five years ago, since which time it had remained in a well-stoppered bottle: it had become but little carbonated or changed in appearance from its original early darkening of colour. While recently preparing the sulphovinic acid of a wax-alcohol, I had occasion to employ this specimen of potash for the neutralization of the sulphuric acid in which the wax-alcohol had been dissolved; more alcohol was added and then the potash. In the mass of sulphate of potassium which separated was found the sulphovinate I was seeking to prepare; but the clear alcoholic filtrate in which nothing was suspected to exist, left, when its alcohol was distilled off, a small quantity of a salt which, after two crystalli-

\* Gmelin (Cav. Soc.), vol. viii. pp. 168, 416.

† Phil. Mag. July 1855 and January 1856.

zations from alcohol, yielded long prismatic crystals, colourless and transparent, but presenting a silky lustre when dried and matted together: they were perfectly soluble in alcohol, and contained no adherent sulphate of potassium, for they yielded no precipitate with chloride of barium until after their ignition. With this substance the experiments subsequently to be mentioned were made.

It could scarcely be doubted that the body originated with the alcoholic solution of potash; yet in order to avoid the chance of error having crept in by means of the complication with the wax-alcohol, or the ethylic alcohol with which its sulphuric solution had been diluted before the neutralization, the remaining portion of the original alcoholic solution of potash was neutralized by simply adding it to some pure sulphuric acid diluted with water to prevent any great rise of temperature. The filtrate from the sulphate of potassium was then distilled; alcohol with a slight odour of aldehyde came over, and the residue, when crystallized twice from alcohol, presented precisely the same features as the salt formerly described. The substance therefore obviously came from the alcoholic solution of potash. This specimen I have not analyzed; for the quantities I had to deal with being small, I was anxious to preserve it for future experiments, or until I could produce it from solution of potash in alcohol at will, an endeavour in which I have not hitherto been successful. The parathionate of potassium does not appear to have been prepared by those who have worked upon these substances, but I hope soon to be able strictly to compare it with the salt which I have thus obtained.

The observations which I have made upon the first-named specimen of this salt are as follows. The substance undergoes no change, nor does it attract moisture when exposed to the air. It crystallizes well from alcohol, whether aqueous or absolute, in long colourless prisms, which present the appearance of silky needles when dried. These crystals withstand a temperature of  $185^{\circ}$  C. without decomposition or fusion; and inasmuch as a portion heated above that temperature underwent no loss of weight, it may be inferred that it contains no water of crystallization. It is very soluble in water; and by the spontaneous evaporation of its aqueous solution the original substance separates without increase of weight, and therefore in the anhydrous form. Its aqueous solution produces no precipitate in solutions of barium, lead, silver, cupric, mercurous, or mercuric salts. At  $188^{\circ}$  to  $190^{\circ}$  C. it fuses, and bubbles of gas form simultaneously in the mass; slight vinous and oily odours are perceptible, and a few drops of liquid condense which are neutral to test-paper. As the temperature rises, the substance undergoes a very slight

darkening; and the white residue is quite soluble in water, contains no sulphurous, but abundance of sulphuric acid; its solution is strongly acid to test-paper, it is in fact anhydro-sulphate of potassium. A portion fused with solid potash yielded no alcohol, but gaseous products of very peculiar odour; this experiment, however, I hope to repeat in a more extended examination.

The salt was crystallized for the last time from almost absolute alcohol, in which it formed a nearly clear solution: the crystals, when dissolved in water, gave no precipitate with chloride of barium, showing that sulphate of potassium had been separated. Yet some impurity has, I fear, remained behind, which has to a certain extent vitiated the analytical results.

Careful combustions were made, the substance being burnt in a platinum boat in a stream of air and subsequently of oxygen; and in one instance the residue in the boat served for the determination of the potassium. The sulphur-determinations were made by oxidizing the substance by concentrated nitric acid (in which it dissolved perfectly in the cold), boiling it frequently with the addition of a few drops of hydrochloric acid and precipitating as barium salt—in the second analysis, evaporating the whole to dryness and igniting in the presence of the chloride and nitrate of barium. The sulphate of barium was freed from oxide from the decomposition of adherent nitrate before the final weighing. In this manner the following results were obtained:—

I. .3687 grm. of substance gave .1794 grm. of  $\text{CO}^2$ , and .0962 grm. of  $\text{H}^2\text{O}$ : it also left a residue of .208 grm., which gave .543 grm. of  $\text{K Pt Cl}^3$ .

II. .5016 grm. of substance gave .2450 grm. of  $\text{CO}^2$ , and .1311 grm. of  $\text{H}^2\text{O}$ .

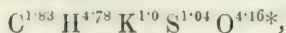
III. .215 grm. of substance gave .318 grm. of  $\text{Ba}^2\text{SO}^4$ .

IV. .160 grm. of substance gave .235 grm. of  $\text{Ba}^2\text{SO}^4$ .

These numbers lead to the following percentage composition, which may here be conveniently compared with that of sulphethylate, or parathionate of potassium:—

	I.	I.	III.	IV.	Mean.	$\text{C}^2\text{H}^6\text{KSO}^4$ .
C . .	13·27	13·23	...	...	13·25	14·63
H . .	2·89	2·88	...	...	2·88	3·05
K . .	23·51	...	...	...	23·51	23·78
S . .	...	...	20·30	20·08	20·19	19·51
O . .	...	...	...	...	40·38	39·03
					100·21	100·00

and hence arises the formula



\* C=12, H=1, K=39, S=32, O=16.



in which the carbon is to the hydrogen in the ratio of 2 : 5.22 ; and thus, notwithstanding the discrepancy in the carbon, I cannot but think that the substance is an isomer of sulphethyrate of potassium. It is not ordinary sulphethyrate, nor is it isethionate. Several circumstances in the foregoing account correspond with the behaviour of neither of these salts ; and it is especially distinct from either in fusing at 188° to 190° C.,—the sulphethyrate\* beginning to melt at 100°, the isethionate not fusing† until between 300° and 350° C. Although therefore I cannot yet consider its identity established, it seems probable that it is the parathionate or parasulphethyrate of potassium.

It would appear, if we carry out the ideas of the chemists who have been working so largely on the propyle and amyle compounds, and by their experiments interpret the fact of the isomerism in these lower members of the alcohol series, that we must admit at least two kinds of arrangement of the atoms constituting the group  $C^n H^{2n+1}$ , one of which may be crudely expressed by assuming the existence of  $C^n H^{2n}$  as a separate grouping, the atom of the radical then becoming  $C^n H^{2n} \cdot H$ .

To this the very facts of the case seem to point. Wurtz‡ obtains his isomer of iodide or bromide of amyle by simply uniting hydriodic or hydrobromic acid with amylene. Decomposed by moist oxide of silver they yield the isomer of the alcohol, which by treatment with sulphuric acid, even in the cold, yields no sulphamylie acid, but di- and tri-amylene. This appears to speak positively to the integrity of the group  $C^5 H^{10}$  throughout these changes. The triple isomerism of the propyle alcohol, again, appears to be intimately bound up with the mode of its production ; that boiling at 96° C.§ results from fermentation. Friedel's|| modification, which boils at 86° to 88° C., is obtained by the hydrogenation of acetone ; whilst Berthelot's from propylene, with which Boutlerow¶ thinks his variety, obtained in the complicated action of chloride of carbonyl on zincmethyle, to be identical, has a boiling-point of 81° to 82° C.

And to return to the sulphovinic series, it is found that when ethylene is combined with sulphuric acid, it is the  $\beta$ -sulphethyrate or parathionate which is obtained. No chemist at the present day would lay too much stress upon the pre-formation of a molecule influencing subsequent molecular arrangement ; but when these instances of isomerism occur in cases in which the proof of metamerism is difficult to obtain, it may be well to take

\* Gmelin (Cav. Soc.), vol. viii. p. 420.

† *Répertoire de Chimie pure* (1862), p. 397.

§ Kekulé, vol. i. p. 431.

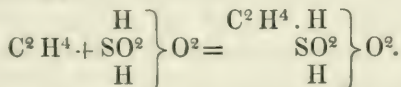
|| *Répertoire de Chimie pure* (1862), p. 352.

† Ibid. p. 430.

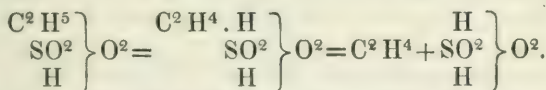
¶ Ibid. 1863, p. 593.

into account every known difference in the genesis of a body in the hope of obtaining thence some elucidation of its structure.

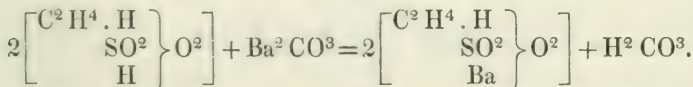
I cannot but think, then, that in the original reaction of Hennel and Berthelot is to be found the cause of the difference in the sulphovinic acids, and that in all cases in which the stable varieties are produced, whether directly or as the result of decomposition, the molecule  $C^n H^{2n}$  may be traced in their structure, whilst in the unstable ones some other arrangement may be found to exist. The reaction of ethylene on sulphuric acid would therefore appear thus :



Berthelot\* has expressed his belief that the althionic acid of Regnault, to which he assigned the same formula as that of sulphethylic acid, is identical with the stable sulphethylic acid, the parathionic acid of Gerhardt; and by what means was it obtained? By heating a mixture of alcohol and oil of vitriol to the point at which it began to evolve olefiant gas, and then arresting the action by saturating with a carbonate. Now we know that alcohol and oil of vitriol at a moderate temperature produce ordinary sulphethylic acid, which at an augmented temperature decomposes, and, we may suppose, with an intermediate stage,



But the final action is in this case arrested by saturation—probably, however, not before the molecular disposition which gives the stable sulphethyrate is brought about; and the carbonate then acts upon the modified acid :

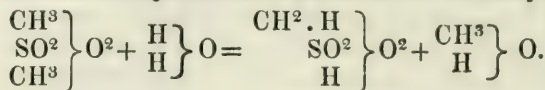


There is yet that pair of reactions productive of the stable sulphovinates which may be represented by the action of water on the sulphate of methyle and on the sulphomethyrate of barium; and of these the first alone needs consideration, since the experiments of Church† have made it almost certain that the first result of the action of water on the latter salt is the production of sulphate of methyle. The action of water on sulphate of methyle is to produce methylic alcohol and  $\beta$ -sulphomethylic

\* *Jahresbericht* (1855), p. 603.

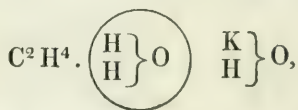
† *Phil. Mag.* January 1856.

acid; and it is not difficult to conceive this change to be due generally to the tendency of complex molecules to assume a simpler form, and in particular to the same striving, as it were, of the atoms of hydrogen and sulphuryle to approximate more closely to that of sulphuric acid, which we have already seen :



The sulphuryle in this decomposition has obtained one atom of hydrogen. May it not induce such a partial separation of another atom from the molecule  $\text{CH}^3$  as shall suffice to constitute the modified sulphomethylic acid? That the alcohol separated is of the ordinary form is shown by its boiling-point; but whether the ordinary form or an isomer be produced, appears to be dependent upon the kind of change involved. If an atom of methyle interchange with an atom of hydrogen of the water, the result would be the ordinary alcohol; if, on the contrary, the atom of methyle decompose, and, leaving an atom of hydrogen behind, should pass over as methylene to the water, then a modification would be produced such as Wurtz's isomer of the amyle alcohol.

And finally comes the case to which I would draw attention—the long-continued action of potash on absolute alcohol. Does it not resemble that of the oil of vitriol in Regnault's experiment? Recently made solution of potash in alcohol yields, so far as my observations have gone, no salt of this kind; but may it not be thought that, after a considerable lapse of time, with the powerful affinity of potash for water constantly present to the molecule of alcohol, some portion is thrown into a state of different molecular arrangement which might almost be called polar?



the integrity of the molecule of alcohol being by no means broken up, but yet so changed in arrangement as to yield, when brought into contact with sulphuric acid, a compound other than ordinary sulphethylete. To this also the fact bears testimony that it is obtained by simple saturation with but the slightest rise of temperature, just as parathionic acid is produced when oil of vitriol at the ordinary temperature absorbs olefant gas; whereas to produce ordinary sulphethylic acid, some length of contact or application of heat is necessary. It might be objected that, if this view were correct, Wurtz's isomer of amyle alcohol



ought to yield with sulphuric acid  $\beta$ -sulphamylic acid instead of producing di- and tri-amylene. But to this surely it might be sufficiently answered, that too close a parallel must not be drawn between the members of homologous series, and that much in the amyle series may depend upon the remarkable tendency which it has to produce polymeric olefines. It appears to me that, placing isethionic acid\* aside, as constructed on a different and mixed type, in which ethylene functions as a diatomic radical, the difference in the sulphethylic acids may be structurally expressed by supposing in one the existence of the molecule ethyle, and in the other of the body  $C^2H^4.H$ , the subhydride of ethylene, both playing the part of monatomic molecules.

XXXIX. On Differential Covariants. By His Honour Chief Justice COCKLE, M.A., F.R.A.S., F.C.P.S., &c., President of the Philosophical Society of Queensland†.

IN my paper entitled "Correlations of Analysis," published in this Journal‡, I showed that in the theory of linear differential equations the factorial substitution leads to results which are analogous to those obtained by the linear substitution in algebra. That we may pursue these analogies, take a symbol  $\delta$ , such that

$$\delta \frac{d^m y}{dx^m} = m \frac{d^{m-1} y}{dx^{m-1}},$$

and consider the differential quantic

$$(1, b, c \chi \frac{d}{dx}, 1)^2 y,$$

whereof the quadratic critical function is

$$b^2 - c + b',$$

the accent denoting differentiation with respect to the independent variable  $x$ . Then, as in algebra the evanescence of the discriminant shows that a quadric and its first derived function are simultaneous, so the condition

$$b^2 - c + b' = 0$$

indicates that the above differential quadric and its derived function

$$\delta(1, b, c \chi \frac{d}{dx}, 1)^2 \text{ or } 2(1, b \chi \frac{d}{dx}, 1)$$

vanish simultaneously.

\* Gorup Besanez, vol. ii. p. 167.

† Communicated by the Author.

‡ Phil. Mag. S. 4. vol. xxiv. p. 531.

Next, consider the differential cubic

$$(1, b, c, f \frown \frac{d}{dx}, 1)^3 y,$$

and, to that end, let  $\Delta$  denote the operation

$$\delta - \frac{d}{db} - 2b \frac{d}{dc} - 3c \frac{d}{df},$$

in which  $\delta$  operates only on  $\left(\frac{d}{dx}\right)^m$  or  $\frac{d^m y}{dx^m}$ , and which gives rise to such results as

$$\frac{db'}{db} = \frac{d}{db} \cdot \frac{d}{dx} \cdot b = \frac{d}{dx} \cdot \frac{db}{db} = 0,$$

and

$$b \frac{d}{dc} (bc') = b \frac{d}{dx} \left( b \frac{dc}{dc} \right) = bb'.$$

Moreover let  $K_2$  and  $K_3$  represent the differential critical functions of the second and third order, so that

$$K_2 = b^2 - c + b',$$

$$K_3 = 2b^3 - 3bc + f - b''.$$

Then, first, we see that the operation  $\Delta$  reduces each of these critical functions to zero, in other words, that

$$\Delta K_2 = 0, \quad \Delta K_3 = 0;$$

and, in the second place we see that if

$$p = 2K_2 b - (K_3 + K'_2),$$

and

$$q = K_2 c - (K_3 + K'_2) b,$$

then also

$$\Delta \left( K_2 \frac{d^2}{dx^2} + p \frac{d}{dx} + q \right) y = 0;$$

or, in other language, the operator  $\Delta$  reduces the expression

$$(K_2, \frac{1}{2} p, q \frown \frac{d}{dx}, 1)^2 y$$

to zero, and that expression is a differential covariant of the given differential cubic.

Now, introduce the factorial substitution of  $uv$  for  $y$ , and divide the given cubic and the covariant, just obtained, by  $u$ , and denote the respective results by

$$(1, B, C, F \frown \frac{d}{dx}, 1)^3 v$$

and

$$(K_2, \frac{1}{2} P, Q \frown \frac{d}{dx}, 1)^2 v.$$

Then we have

$$P = p + 2K_2 \frac{u'}{u},$$

$$Q = q + K_2 \frac{u''}{u} + p \frac{u'}{u}.$$

Again, in  $p$  and  $q$  substitute  $B$ ,  $C$ , and  $F$  for  $b$ ,  $c$ , and  $f$  respectively, and call the results  $(P)$  and  $(Q)$ . Then

$$(P) = 2K_2 B - (K_3 + K'_2)$$

$$= p + 2K_2 \frac{u'}{u},$$

$$(Q) = K_2 C - (K_3 + K'_2) B$$

$$= K_2 \left( \frac{u''}{u} + 2b \frac{u'}{u} + c \right) - (K_3 + K'_2) \left( b + \frac{u'}{u} \right)$$

$$= q + K_2 \frac{u''}{u} + p \frac{u'}{u},$$

and, consequently,

$$(P) = P, \quad (Q) = Q.$$

Hence the expression

$$\left( K_2 \frac{1}{2} p, q \chi \frac{d}{dx}, 1 \right)^2 y$$

is properly termed a differential covariant of the given differential cubic; for when the  $y$  in both is transformed by the same factorial substitution, then the result obtained by transforming that expression is the same as the result obtained by forming an expression,

$$\left( K_2, \frac{1}{2} P, Q \chi \frac{d}{dx}, 1 \right)^2 v,$$

in which  $P$  and  $Q$  respectively are the same functions of  $B$ ,  $C$ , and  $F$  that  $p$  and  $q$  are of  $b$ ,  $c$ , and  $f$ .

It will be remembered that a critical function remains unaltered after factorial substitution (to which the division by  $u$  is an accompaniment), and that the differentials, with respect to the independent variable  $x$ , of critical functions are critical.

That  $\Delta$  causes this covariant to vanish is readily shown; for

$$\Delta(K_2 y'' + p y' + q y) =$$

$$(2K_2 + \Delta p) y' + (v + \Delta q) y,$$

and

$$2K_2 + \Delta p = 0, \text{ and } p + \Delta q = 0.$$

It may be convenient to exhibit the differential cubic and the differential covariant above obtained in another shape. Let, then, the cubic be written thus:

$$\frac{d^3 y}{dx^3} + b \frac{d^2 y}{dx^2} + c \frac{dy}{dx} + f y;$$



then the above differential covariant may be written

$$\begin{aligned} & (b^2 - c + b') \frac{d^2 y}{dx^2} \\ & + (bc - f + c') \frac{dy}{dx} \\ & + (-2b^4 + 4b^2c - 2b^2b' - bf + bc' + cb' - c^2)y. \end{aligned}$$

The binordinary differential critical function of this covariant is

$$K_2 + \left( \frac{K_3 + K'_2}{2K_2} \right)^2 - \frac{d}{dx} \left( \frac{K_3 + K'_2}{2K_2} \right),$$

and is the same as that of

$$y'' - \left( \frac{K_3 + K'_2}{2K_2} \right) y' - K_2 y,$$

the accents denoting differentiation with respect to the independent variable  $x$ . Each of the coefficients of this function is critical.

The parallelism between some of the foregoing results and those of the algebraical covariant theory will perhaps be better seen if we consider the complete form

$$(a, b, c, f \chi \frac{d}{dx}, 1)^3 y.$$

For this form we have, say,

$$\begin{aligned} K_2 &= b^2 - ac + ab' - ba', \\ K_3 &= 2b^3 - 3abc + a^2f - a(ab'' - ba'') + 2a'(ab' - ba'), \end{aligned}$$

and the second and last coefficients of the differential covariant obtained in this paper become, respectively,

$$\begin{aligned} p &= bc - af + ac' - ca', \\ q &= c^2 - bf + bc' - cb' - 2(K_2 - b')K_2. \end{aligned}$$

The resemblance of these expressions to those which occur in the ordinary Hessian, though not perfect, is marked. I first deduced the differential Hessian (for the case  $a=1$ ) by seeking an expression for which the coefficients of transformation and of substitution should be the same, and I afterwards found that an operator, to wit  $\Delta$ , reduced it to zero. I take this opportunity of referring (in connexion with binordinary differential critical functions) to my three other papers "On Linear Differential Equations of the Second Order" in the first volume of the Oxford, Cambridge, and Dublin Messenger of Mathematics.

Brisbane, Queensland, Australia,

December 11, 1863.

## XL. Notices respecting New Books.

*Researches on the Solar Spectrum and the Spectra of the Chemical Elements.* By G. KIRCHHOFF.—Second Part. Translated by H. E. ROSCOE, F.R.S., &c. Macmillan and Co., 1863.

IN this second paper we have the completion of Prof. Kirchhoff's survey of the solar spectrum, furnishing us with maps and catalogues of the lines in the extreme parts of the spectrum from A to D, and from near F to G. We have before had the pleasure of noticing the first memoir treating of the intermediate region of the spectrum. In his extended survey, indeed, Kirchhoff was obliged to commit the practical observation to his pupil Mr. G. Hofmann, his own eyesight having been unfortunately weakened by constant application to the spectroscope. But the method of observation is the same as before.

We may again point out that these maps are indispensable to any observer of the spectrum. There are great numbers of scientific men now independently engaged upon the subject, and there will be vast confusion and loss of labour unless all observations be referred to one uniform scale or ground-plan, such as it seems to be the chief purpose of these maps to afford. Though the wave-length is the ultimate criterion of position of a spectral line, the variation of refracting power deprives us of any natural scale of practical reference. Kirchhoff's arbitrary scale should then be adopted by general consent, both in deference to him, and in order to avoid the confusion of nomenclature or notation in which this rising and beautiful science will otherwise be involved.

From the text we learn that the evidence for the existence of potassium in the sun's atmosphere has broken down under closer examination, but that additional evidence has been obtained of the existence of iron, nickel, barium, copper, zinc, strontium, cadmium, &c. No additional elements have been found in the sun.

## XLI. Proceedings of Learned Societies.

### ROYAL SOCIETY.

[Continued from p. 154.]

April 16, 1863.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair.

THE following communications were read:—

“On Ozone.” By E. J. Lowe, Esq., F.R.A.S., F.L.S.

This paper consists of two parts, viz.:—

1. On the precautions necessary in ozone observations, and on certain corrections requisite before the actual amount can be determined.
2. The discovery of dry ozone powders as a substitute for the ordinary tests; an investigation into the ozone paper tests of M. Schönbein and Dr. Moffat, the determination of a proper formula for the tests, with an account of various observations and experiments made on the subject.

## PART I.

At the last Meeting of the British Association I read a paper on the precautions and corrections requisite in order that a more perfect knowledge of ozone might be obtained. These precautions comprise uniformity of observation, each observer using the same box and the same tests, suspended at the same height, and as nearly as possible placed under the same circumstances. The corrections necessary are:—1st, For the velocity of the air; 2nd, for the height of the barometer; 3rd, for temperature; 4th, for the hygrometrical state of the air; 5th, for elevation above the ground.

1. *Velocity of the Air.*—The greater the speed the more ozone will be apparent, and this seems to be owing more to the increased velocity of the air than to a greater proportion of ozone.

2. *Height of the Barometer.*—It is found that during the last four years,

With the barometer at  $28\frac{1}{2}$  inches the amount of ozone was 5·7

”	”	29	”	”	”	3·5
”	”	$29\frac{1}{2}$	”	”	”	2·0
”	”	30	”	”	”	1·3
”	”	$30\frac{1}{2}$	”	”	”	0·4

A law as regards ozone and pressure is clearly apparent; but as the barometer falls for wind, the excess at low pressures is no doubt partly due to the increased velocity of the air. There is more ozone with the wind between W.S.W. and S.S.E. than when between N.N.W. and E.N.E., and the barometer is half an inch lower with S.W. winds than with N.E. winds.

3. *Temperature.*—Temperatures between  $30^{\circ}$  and  $40^{\circ}$  will give less ozone than when between  $40^{\circ}$  and  $50^{\circ}$ , and the latter less than when between  $50^{\circ}$  and  $60^{\circ}$ . The same holds good when the ozone box is artificially warmed. This does not extend to very high temperatures, because the great dryness of hot weather is against the action of ozone on the tests.

4. *Moisture.*—Increase of moisture up to a certain point is favourable to the colouring of the tests, beyond which it operates unfavourably; for when the air is completely saturated with moisture, the effect of ozone is at its minimum.

5. *Altitude.*—The higher the test is hung the darker will be the colour obtained. The difference is as 4 to 6 between 4 feet and 35 feet above the ground.

There are yet several other circumstances to be mentioned:—

1. *Hour of the Day.*—The difference between the ozone readings at night and in the daytime are—

In June and July an excess at night of	0·1
In August and September	” 0·4
In October and November	” 0·5
In December and January	” 0·8
In February and March	” 0·7
In April and May	” 0·7

the average excess of the summer months being only one-half of that which occurs in winter.



2. *Direction of the Wind.*—There is most ozone with the wind between S. and S.W., and least when between N. and N.E.

3. *Protection of the test from light.*—It is absolutely requisite that the test should be in a dark box; and no box has been found to answer so well as that constructed by myself, and known as "Lowe's Ozone box," an account of which has been furnished to the Royal Society\* and to the British Association. This box, if freely exposed, and made to veer with the wind, so as always to present the opening to the direct current, is everything that could be desired.

The foregoing observations will be sufficient to show that precautions are requisite in these records, and that certain corrections are necessary before we can declare whether ozone is present in a certain fixed amount, or whether it changes from hour to hour. These corrections have yet to be found out; those for the height of the barometer and the force and direction of the wind will be considerable.

## PART II.

The ozone tests heretofore used have appeared to me to be unsatisfactory, and, on close examination, I found them to be faulty in many respects. The paper used had a glaze upon it, which prevented the solution from penetrating it; substances, moreover, had been used in its manufacture which acted injuriously on the tests. Again, the starch of commerce was found to be impure; it is manufactured with lime, sulphuric acid, and chlorine, substances fatal to these tests. The iodide of potassium was also impure; and there has been a want of uniformity in the proportions of starch and iodide of potassium employed by different observers.

Having found out that the starch of commerce was impure, I procured a jar of wheat-starch in the wet state before any chemicals had been used. This was steeped in distilled water, which was changed every two days until quite sweet to the taste, and, although by a long process, a chemically pure starch was thus obtained.

Sir John Herschel suggested trying other vegetable starches; I therefore made starch from rice, potato, sago, and wheat.

I obtained chemically pure iodide of potassium from Mr. Squire of Oxford Street, who forwarded me two samples made expressly for these experiments, the one prepared with water, the other crystallized several times from alcohol.

On the recommendation of Dr. R. D. Thomson, 15 grains of prepared chalk have been added to each ounce of air-dried starch to prevent it from becoming sour from any moisture that might be contained in it; subsequent observations have proved that this is absolutely requisite for uniformity of effect, as the intensity of action depends upon the amount of water contained in the starch, which is apparent from the following experiment:—

Tests made with air-dried starch—

α. Without further drying became coloured in 5 minutes.

β. After further drying by fire-heat for 1 minute became coloured in 7 minutes.

\* Phil. Mag. S. 4. vol. xxi. p. 466.

γ. After further drying by fire-heat for 3 minutes became coloured in 9 minutes.

δ. After further drying by fire-heat for 10 minutes became coloured in 13 minutes.

ε. After further drying by fire-heat for 30 minutes became coloured in 20 minutes.

η. With chalk added became coloured in 20 minutes.

With regard to the calico or paper used for the tests, both stained when impure. However, Mr. Joseph Sidebotham of the Strine Works prepared for me some chemically pure calico, and I was also enabled to procure a very porous chemically pure paper, both of which answer perfectly.

Having succeeded with the ozone slip tests, I tried as a first experiment a mixture of 10 parts of starch to 1 of iodide of potassium as a "dry-powder test;" this, when well mixed in a mortar, was bottled ready for use. A small portion was placed in the open air, and ten minutes' exposure showed that powder tests were an undoubted success, being more sensitive than the test slips. My next determination was what strength would colour quickest, and accordingly a number of strengths were prepared, varying in the proportions from 1 of iodide of potassium and 1 of starch up to 1 of iodide of potassium and 30 of starch, the starch used being made from wheat. From these experiments it was found that the proportion of 1 of iodide of potassium to 5 of starch was invariably the darkest, the degree of darkness diminishing in either direction when other strengths were used; thus 1 of iodide of potassium to  $4\frac{1}{2}$  of starch, or 1 to  $5\frac{1}{2}$ , were neither so dark as with a strength of 1 to 5.

On repeating these experiments with potato-starch, the proportion that coloured soonest was 1 to  $2\frac{1}{2}$ ; and this second series of experiments proved that with each starch a special formula is requisite.

My next experiments were with the view of ascertaining the effect of various acids and chemical substances on the ozone powder tests. For this purpose I procured a number of cups for solutions, and small pill-boxes to hold the powder tests, and these were placed together under separate bell-glasses. The result was that the following coloured the powder tests very rapidly:—Hydrochloric acid, nitric acid, nitrous acid, chloride of lime, phosphorus, iodine (in scales), iodine (dissolved in alcohol), carbonate of iron on which sulphuric acid was poured, carbonate of iron on which glacial acetic acid was poured, limestone on which sulphuric acid was poured, limestone on which glacial acetic acid was poured, matches lighted under the bell-glasses. The following did not colour the tests:—Sulphuric acid, glacial acetic acid, carbonate of lime, carbonate of iron, ammonia, matches not lighted.

The substances used in the manufacture of ordinary starch of commerce gave the following:—

Chloride of lime coloured the tests instantaneously.

Sulphuric acid did not colour the tests.

Lime did not colour the tests.

Lime and sulphuric acid mixed coloured the tests rapidly.

There are advantages in the powders over the ordinary tests. They are more sensitive, and therefore more rapidly acted upon; they retain their maximum colour, not afterwards fading, as with the tests of Schönbein and Moffat. (However, my calico and porous-paper tests are not nearly so liable to fade, owing to the solution penetrating into the fabric used, instead of being merely a surface-covering.) There is also a more important advantage still to be mentioned from the use of powders. By the aid of powder tests we shall ascertain what colours the tests; in the experiments it was found that a different colour was imparted to the powder, and that the colour penetrated deeper with some substances and acids than with others, so that differences of effect took place, from which the different materials used might be recognized. Thus:—

1. Iodine, although coloured a brown-black, was merely a surface colouring, below the powder remained colourless.

2. Phosphorus, bluish black on the surface only, below almost colourless.

3. Chloride of lime, deep brown on the surface only, the powder below slightly yellow.

4. Hydrochloric acid, grey-pink on the surface only, the powder beneath orange.

5. Nitric acid, dark-red brown extending slightly into the powder, beneath that colourless.

6. Carbonate of iron with glacial acetic acid, yellowish brown to the thickness of cardboard, below that buff.

7. Limestone with sulphuric acid, pale brown to the thickness of cardboard, beneath slightly coloured.

8. Carbonate of iron with sulphuric acid, black to the depth of a quarter of an inch.

9. Nitrous acid, dark brown more than the eighth of an inch deep, beneath yellowish brown.

10. Nitric acid mixed with exposed ozone powder, blue-black to the sixth of an inch deep, below that reddish brown.

11. Nitric acid mixed with unexposed ozone powder, blue-black to the sixth of an inch deep, below that reddish brown.

These experiments may require some modification, yet they point out the fact that striking differences are apparent, differences which must open up a new method of investigating ozone.

Not only have the tests hitherto used been made without due regard to the pureness of the chemicals and fitness of the material used, but the paper box in which they have been kept is not sufficient for their perfect preservation; a dark, dry, air-tight box is essential; and this should not be opened in a room where there is iodine, chlorine, nitric acid, phosphorus, hydrochloric acid, or other chemicals likely to be injurious to the tests. I am now manufacturing the tests, which will be distributed by Messrs. Negretti and Zambra, and I have constructed a proper box in which in future they will be sent.

“On the Equations of Rotation of a Solid Body about a fixed Point.” By William Spottiswoode, M.A., F.R.S.

“On the Fossil Human Jawbone recently discovered in the Gravel near Abbeville.” By W. B. Carpenter, M.D., V.P.R.S.



April 23.—Major-General Sabine, President, in the Chair.

The following communication was read:—

“On the Diurnal Inequalities of Terrestrial Magnetism, as deduced from observations made at the Royal Observatory, Greenwich, from 1841 to 1857.” By George Biddell Airy, F.R.S., Astronomer Royal.

The author describes this paper as one of the class which gives the epitomized results of long series of voluminous observations and laborious calculations, of which the fundamental details have been printed in works specially devoted to these subjects. It exhibits in curves the diurnal inequalities of terrestrial magnetism, as obtained by the use of instruments essentially the same, through the whole period of seventeen years, during the last ten years of which the magnetic indications have been automatically recorded by photographic self-registration, on a system which has been continued to the present time, and is still to be continued.

From the last months of 1840 to the end of 1847, the observations were made by eye, every two hours. From the beginning of 1848, for the declination and horizontal force magnetometers, and from the beginning of 1849, for the vertical force magnetometer, the magnetic indications are recorded by Mr. Brooke's photographic apparatus.

In preparing the reductions of the magnetic records from 1848 to 1857 (which are printed in the “Results of Magnetical and Meteorological Observations for 1859,” bound in the volume of ‘Greenwich Observations,’ 1859, and also issued separately), the days of unusual magnetic disturbance had been separated from the rest, and the reductions applied to the mass so diminished. For unity of plan, it appeared expedient to follow the same course for the reductions from 1841 to 1847. In consequence of this, the numbers which are used here differ in some cases by small quantities from those printed in the ‘Greenwich Magnetical Observations from 1841 to 1847.’ The numbers in the reductions from 1848 to 1857 are adopted without change.

The author remarks that, taking the number of omitted days as a rough measure of the amount of magnetic disturbance, there is no appearance of decennial cycle in their recurrence, and no distinct relation to the magnitude of diurnal changes.

The author then proceeds to the description of the curves. The first four sheets contain the curves in which the horizontal abscissa represents the declination at each hour as compared with the mean for the twenty-four hours, and the vertical ordinate represents the horizontal force at each hour as compared with the mean for the twenty-four hours. On the different sheets the days are differently grouped, thus:—On sheet I. all the observations at each nominal hour throughout the year are combined; this sheet contains the separate curves for 1841, 1842, 1843, 1844, 1845, 1846, 1847. On sheet II. similar curves are formed for 1848, 1849, 1850, 1851, 1852, 1853, 1854, 1855, 1856, 1857. On sheet III. all the observations at each nominal hour through all the months January from 1841 to 1847 are combined to form the January curve; all those

through the months February to form the February curve, and so on. On sheet IV. similar month-curves are formed from the period 1848 to 1857. It is remarked that the origin of coordinates necessarily represents the mean declination and mean horizontal force in each month.

The author then points out that the means for each month are themselves subject to an annual inequality, which can be ascertained with little difficulty. The values of these inequalities are exhibited, for declination and horizontal force, separately for the period 1841-1847 and for the period 1848-1857; those in the first period far exceed in magnitude those in the second (as holds also with regard to all the diurnal inequalities).

If we wished to exhibit the hourly state of magnetism, as referred to the mean state given by the supposition of uniform secular change of normal magnetism, we ought to apply these quantities with sign changed, to the origin of coordinates in each curve, in order to form a new origin of coordinates. For the year-curves, the numbers destroy each other, and no new origin of coordinates is produced; for the month-curves, however, they shift the origin materially. The author does not perceive that any facility for theoretical reference or other advantage is gained by this step.

On examining the year-curves, it is seen that from 1841 to 1848 their magnitude very slowly increases, with a small change of form, but from 1848 to 1857 their magnitude very rapidly diminishes, with a great change of form. Some great cosmical change seems to have come upon the earth, particularly affecting terrestrial magnetism. On comparing these year-curves with the month-curves, especially with those for the period 1848-1857, it appears that the change of the year-curves from 1848 to 1857 nearly resembles that of the month-curves from summer to winter; and the author points out as a possible step to a physical explanation of the change from 1848 to 1857, that the magnetic action of the sun upon the earth's southern hemisphere may have remained nearly unaltered, while that on the northern hemisphere may have undergone a great diminution.

The author then alludes to the curves representing the hourly state of vertical force, as referred to the mean on each day. The force is here represented by a simple ordinate. The grouping is made by years and by months in the same manner as for the curves already mentioned. The month-curves of the two periods (1841-1847 and 1848-1857) differ, in the magnitude and change of magnitude of the ordinates, and in the place and change of place of node. The year-curves of the two periods have some very remarkable differences. From 1847 to 1849 the magnitude of the ordinates increases sensibly; from 1849 to 1850 still more; it then remains nearly stationary. In 1846 the descending node is at  $11^{\frac{3}{4}}$  nearly; in 1847 it is at  $9^h$  nearly; in 1849 at  $7^h$  nearly; in 1850 at  $5^h$ ; in 1851 at  $4^h$ ; and there it continues with little alteration. It is important to observe that, though the instrument was changed in 1848, the change in the place of the node did not then occur suddenly; it had begun with the old instrument, and continued to advance gradually for several years with the new instrument. The author states

that he had verified the correctness of the node in the first period from other observations, but he had not succeeded in finding observations corresponding in date with those of the latter period.

The paper is followed by eight sheets of curves, as follows:—

*I. Diurnal Curves of combination of Declination and Horizontal Force.*

- (1) Mean of all the days in each year (separately), 1841–1847.
- (2) Mean of all the days in each year (separately), 1848–1857.
- (3) Mean of all the days in the aggregate of the same nominal months (separately) through the period 1841–1847.
- (4) Mean of all the days in the aggregate of the same nominal months (separately) through the period 1848–1857.

*II. Diurnal Curves of Vertical Force.*

- (5) Mean of all the days in each year (separately), 1841–1847.
- (6) Mean of all the days in each year (separately), 1849–1857.
- (7) Mean of all the days in the aggregate of the same nominal months (separately) through the period 1841–1847.
- (8) Mean of all the days in the aggregate of the same nominal months (separately) through the period 1849–1857.

*XLII. Intelligence and Miscellaneous Articles.*

ON THE EARTH'S CLIMATE IN PALÆOZOIC TIMES.

*To Dr. John Tyndall, F.R.S., &c.*

MY DEAR SIR,

Montreal, February 1, 1864.

**I**N a Note which appeared in the Philosophical Magazine for October 1863 (page 323), I endeavoured to explain the warmer climate of the earth in palæozoic times by the aid of your remarkable researches on radiant heat, by which you have shown that small quantities of various gases, when diffused in the atmosphere, greatly retard cooling by radiation. I suggested that the large amount of carbon which was then diffused in the atmosphere as carbonic acid, and which has since been separated in the forms of carbonate of lime and carboniferous matters, must have had a powerful influence upon the temperature of the earth's surface during the palæozoic period; I also indicated the influence of marsh-gas from the decay of the abundant vegetation of the period. I was not at the time aware that my suggestions had been anticipated by you in the Bakerian Lecture for 1861, where you say that, from its influence on terrestrial radiation, all variation in the amount of aqueous vapour in the atmosphere must produce changes in climate; and you then add, "Similar remarks would apply to the carbonic acid diffused through the air, while an almost inappreciable admixture of any of the hydrocarbon vapours would produce great effects on the terrestrial rays, and corresponding changes of climate. It is not, therefore, necessary to assume alterations in the density and height of the atmosphere to account for different amounts of heat being preserved to the earth at different times; a slight change in its variable constituents would



account for this. Such changes, in fact, may have produced all the mutations of climate which the researches of geologists reveal." (*Philosophical Magazine*, Oct. 1861, p. 277.)

In the *American Journal of Science* for November 1863 (p. 398), I have in this connexion referred to a paper by the late Major E. B. Hunt, United States Engineers, "On Terrestrial Thermotics," published in the *Proceedings of the American Association for the Advancement of Science* for 1849. In this paper Major Hunt argues that the temperature of the earth's surface increases with the weight of the barometric column, and that, as the atmospheric mass must have been greater in the earlier geological periods by the amount of carbon and of carbonic acid since abstracted, the temperature of the earth must then have been higher. To this mass Prof. Dana would add the abundant moisture of the insular climate of former times. The augmentation of barometric pressure dependent upon these additions to the atmosphere would, however, probably be very inadequate to explain the considerable differences in climate of which geology affords us evidence; and it is only in the light of your recent discoveries that the chemical constitution of the early atmosphere of our globe enables us to explain the warmth of ancient climates.

The object which I have in writing this note is thus not only to set forth Major Hunt's early attempts to solve the problem, but to call public attention to the fact that what I conceive to be its true solution was first suggested by yourself.

I have the honour to be, my dear Dr. Tyndall,

Very faithfully yours,

T. STERRY HUNT.

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ON THE INTENSITY OF SOLAR RADIATION IN DIFFERENT  
SEASONS. BY FATHER A. SECCHI.

Several meteorologists have introduced the use of a blackened thermometer exposed to the sun for obtaining data on solar radiation. Having myself used this instrument, I am convinced that no certain conclusions can be obtained with it, for the temperature depends on several circumstances which it is impossible to measure specially. It depends in fact, (1) on the direct solar radiation; (2) on the radiation of surrounding objects; (3) on the radiation of the air; (4) on the agitation of the atmosphere. Yet as this element is of great importance, and it is desirable to simplify this class of observations so as to make them commoner in different climates, I have endeavoured to improve this instrument by introducing into its use principles already employed by some other observers, and especially by Mr. Waterston.

My apparatus consists of a metallic reservoir formed of two concentric cylinders of different diameters, the annular space of which is filled with water. The internal cylindrical space is empty, and free and open at both ends to receive the solar rays. In the axis of this cylinder, by means of a tube which traverses the annular space

perpendicularly, a thermometer with a blackened tube is introduced, on which strike the solar rays directed on the axis of the cylinder. A thick glass closes the posterior opening of the cylindrical tube, the other remaining free; but with a diaphragm of suitable aperture the entire apparatus is protected from solar radiation. The entire apparatus is mounted on a parallactic foot provided with very regular movements. For the other details, and for the mode of using this instrument, the *Bulletin de l'Observatoire du Collège Romain*, vol. xi. p. 14, may be referred to. I may simply mention that the thermometer-scales are divided into fifths of a degree.

By means of this arrangement there are eliminated (1) the agitation of the air, and (2) the radiation of surrounding objects, the influence and force of which may always be known, and varied at pleasure by changing the temperature of the water of the envelope. There only remains the radiation of the sun, and that of the portion of the heavens visible from the place of the thermometer, and which can be diminished at will by placing in front of the instrument a diaphragm scarcely larger than the bulb of the thermometer. As regards the influence of the absolute temperature of the envelope, it is well known by Mr. Waterston's observations that it has no influence on the increase of temperature which solar radiation produces on the blackened thermometer; and I have assured myself, by varying the temperature of the water between 7 and 70 degrees, that when the temperature of the two thermometers is stationary, their difference remains constant whatever their absolute value; so that this difference (at least within certain limits) does not depend on the temperature of the envelope. This at first sight appears very singular; for it is curious to see that if the black thermometer rises to  $19^{\circ}6$  when the water is at  $7^{\circ}$ , when the temperature of the latter is at  $70^{\circ}$  the black thermometer rises to  $82^{\circ}$  in the sun, the difference always remaining 12 degrees. The practical difficulty of verifying these facts depends on the rapidity of variation which the thermometer placed in water undergoes for high temperatures; but there is an indirect verification in the fact that, after a certain limit, both thermometers rise together, their difference always remaining constant.

With this apparatus I made a great number of observations during summer, and I have repeated them in the present season in the perfectly bright days from the 22nd of November to the 8th of December, exposing the apparatus to the solar radiation under the dome of the observatory until the relative temperature was constant for a considerable time. The following are the principal conclusions at which I have arrived.

1. During summer, observing near the meridian and near the solstice, the relative temperature has varied from 14 to 11 degrees; the mean of several observations gives  $12^{\circ}06$ .

2. Continuous observations during the month of August give values from 13 to 11 degrees. Their mean is still 12 degrees.

3. Those of November and December give  $12^{\circ}5$  and  $11^{\circ}5$ , and the mean has not appreciably changed.

4. Observing in summer near the horizon, at an elevation of 30 to 34 degrees, the temperature rises merely to 6°·5.

5. The rapidity with which the blackened thermometer mounts is scarcely different in summer and winter up to 10 or 11 degrees; but after this limit the maximum is sooner obtained in summer than in winter.

The results obtained in the latter season were quite unexpected; for I believed I should have in winter, observing with a sun's height of about 28 degrees, a temperature at most equal to what I had in summer at 32 degrees of elevation, for the atmospheric height was almost the same; but it was not so. In the meridian, I obtained almost the same value as in summer, although the rays traversed double the thickness of atmosphere, while this double thickness in summer diminishes the force of radiation and reduces it to the half.

These phenomena would be inexplicable if the absorbing power of aqueous vapour were not known\*. In fact, if we compare observations made at the same height in summer and in winter, we find the radiation of summer one-half, even though the atmospheric thickness is the same; but aqueous vapour, which in summer in our climate has a tension of 13 to 14 millims., in winter has only 7 to 8. It is difficult to estimate the absolute quantity of vapour which exists in the path of the rays, for the psychrometer only gives the quantity near the ground, which differs materially from that above. Moreover, the level of vapour is in summer much higher than in winter. Yet it would not be far from the truth to admit that in summer there is at least double as much as in winter. Thus we find that with an equal height the radiation is reduced to one-half in summer, as the quantity of vapour requires.

Two interesting conclusions follow from these facts:—(1) That in summer the radiation in the meridian is greatly enfeebled by aqueous vapour, so that without it we should have double the radiation (it would be necessary to verify this for high mountains, which would be well worth the trouble). (2) That the absorbing force of vapour is very great; for we see that a quantity of 7 to 8 millims. in excess in summer produces an absorption equal to that of the thickness of the entire atmosphere of gas in winter, which would give for the absorbing force of vapour 95 times that of air. In these calculations we have not allowed for the greater elevation of the vaporous layer in summer, nor for the phenomenon of thermochrose, which causes the first layers to absorb more than the following. But in general we may admit that Mr. Tyndall's result, which makes it sixty times as much as air, is not far from the truth.

My researches on this important subject are not finished; I intend to improve the apparatus and observe in different seasons with greater care; but the first results have been so interesting that, although merely sketched, I thought it right to present them to the Academy. Further researches may elucidate the numerous questions which attach themselves to this subject, and may separate that

\* In winter the sun's radiation is stronger because he is nearer us, but that does not produce a difference of more than  $\frac{1}{16}$ .



which in the daily differences belongs to our planet from that which belongs to the sun itself; but the principal results given here could not be essentially modified. They show how imperfect is our knowledge on the absorption of terrestrial atmosphere, and on the absolute force of solar radiation.—*Comptes Rendus*, January 4, 1864.

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ON THE ELECTRICAL PROPERTIES OF PYROXYLINE-PAPER AND GUN-COTTON.

Prof. John Johnston, of Wesleyan University, Ct., has called my attention to a remarkable power in pyroxyline-paper of producing positive electrical excitement in sulphur, sealing-wax, &c. His note is as follows:—

“Wesleyan University, Middletown,  
December 24, 1863.

“DEAR SIR,—We are told by writers on electricity that sulphur by friction with all other substances becomes *negatively* excited; as cat’s fur, on the other extreme, by friction with all other substances becomes excited *positively*. But a few days ago I made the discovery that sulphur by friction with paper pyroxyline (I will call it) is excited with positive electricity, as are also sealing-wax, amber, &c. The paper is prepared in the same manner as gun-cotton, which would also in all probability be found to possess the same property.

“Enclosed you will find some of the paper for trial. It was prepared by my son, M. M. Johnston.

“Perhaps you will think the matter of sufficient importance to make a note of it in the *Journal of Science*.

“Respectfully yours,

“*Prof. Silliman.*”

“JOHN JOHNSTON.”

I have repeated and confirmed Prof. Johnston’s experiment, extending it to gun-cotton. I find, as he suggests, that the latter substance produces the same excitement of positive electricity which is produced by the pyroxyline-paper. The most energetic effects are produced when vulcanized india-rubber is the electric. The opposite effects in this substance produced by flannel and the gun-cotton or pyroxyline-paper are very striking, and will form a good lecture-room illustration. These substances also produce powerful positive excitement in glass. It is difficult from the use of pith-balls alone to determine which produces the most powerful positive excitement, glass or hard rubber, when excited by gun-cotton or pyroxyline-paper. This seeming anomaly, confounding our ordinary means of discrimination in cases of electrical excitement, demands further investigation. It would appear that of negative electrics yet observed these azotized species of cellulose are the most remarkable—in comparison with which the most highly negative electrics hitherto known become positive.—B. SILLIMAN, Jun.—*Silliman’s American Journal*, January 1864.



Fig. 2.

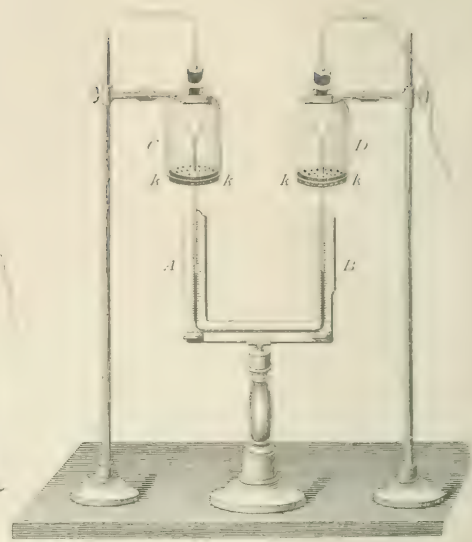


Fig. 1.

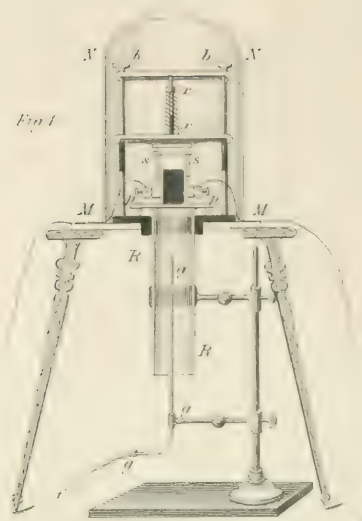
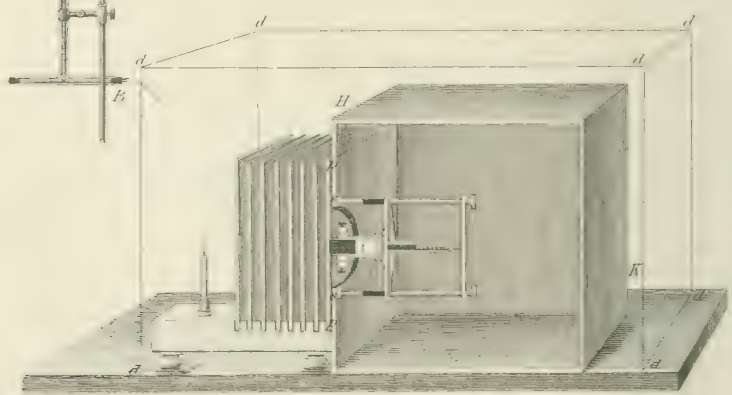


Fig. 3.



Fig. 4.



0 10 20 30 40

Scale to Fig. 1 & 3.

0 10 20 30 40 50 60

Scale to Fig. 2 & 4.



THE  
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[FOURTH SERIES.]

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APRIL 1864.

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XLIII. *On the Condensation of Vapours on the Surfaces of Solid Bodies.* By G. MAGNUS\*.

[With a Plate.]

ON a former occasion† I made the observation that a thermal pile is raised in temperature when moist air of the same temperature with itself comes in contact with it, and that it is cooled when dry air of the same temperature passes over it. This appearance can only be explained, so far as I can see, on the supposition that the surface of the pile condenses watery vapour from the atmosphere and is warmed by the latent heat which is set free, and that the dry air takes water back again from the surface of the pile, whereby the latter is cooled. The pile displayed these effects not only when it was coated with pine-soot or lampblack, but also when its surface was, as far as possible, freed from such coatings, and quite metallic. That powdery substances condense vapour of water and also gases is known, and Jamin and Bertrand‡ have sought to measure this condensation. I have myself§ also proved the condensation of sulphurous acid on the surface of glass; but that a metallic surface should absorb aqueous vapours in such quantity as to occasion an appreciable elevation of temperature was very astonishing; and it seemed to me to be well worth while to investigate this absorption more closely.

\* Translated by Prof. Wanklyn from Poggendorff's *Annalen*, vol. cxxi. part 1 (1864, No. 1), p. 186.

† *Pogg. Ann.* vol. cxviii. p. 575; *Phil. Mag. S. 4.* vol. xxvi. p. 21.

‡ *Comptes Rendus*, vol. xxxvi. p. 994.

§ Poggendorff's *Annalen*, vol. lxxxix. p. 604.

Since the metals of which the thermo-piles are usually constructed possess, from their crystalline structure, a kind of porous nature, it was conceivable that it was in consequence of this that the pile exhibited the phenomena in question. But when thin platinum-foil was laid upon the pile and alternately a current of dry and of moist air directed against it, the same depression and elevation of temperature, only less pronounced, took place in presence of the covering of foil as had been noticed without it. Of necessity, then, the elevation of temperature due to absorption on one side of the foil must have been sufficient to occasion an elevation of temperature on the other side sufficient to affect the pile to an appreciable extent.

The known property of platinum, of causing combination between gases, could not have occasioned this phenomenon, for it soon became apparent that other metals, when they were substituted for platinum, occasioned a similar elevation of temperature.

In order to ensure that the pile did not come into direct relation with the air, and that the rise of temperature was due to the plate, the following apparatus was employed.

The plate *pp* (Plate III. fig. 1), which was to be experimented upon, was laid upon the upper border of an upright glass tube, *RR*, of 150 millims. in length and of 35 millims. in diameter, open at both ends, and provided above with a well-ground rim. Upon this plate was placed the thermo-pile *ss*, which was provided at its upper part with a brass cap, and was pressed against the plate *pp* by means of a spiral spring *xx*. The pile, with the bow *bb*, which served to allow of the fastening of the spiral spring, was covered with a glass shade, *NN*, which was closed below by means of a glass plate *MM*, through which the glass tube *RR* passed. The plates *pp* to be operated upon were quite flat. When they could not be got large enough to close the tube *RR*, then a glass plate with an opening in the middle, somewhat larger than the diameter of the pile, was placed immediately in contact with the tube. The plate to be used was then applied to this glass plate, and the pile pressed against it by means of the spiral *xx*. By means of a bellows the air was forced into the tube *RR* through a caoutchouc tube *fg*, on the end of which there was a glass tube *gg*. This glass tube *gg* was brought vertically into the middle of the tube *RR*, terminating at a distance of 40 millims. below the plate *pp* to be experimented upon, and so the air streamed against the plate from this distance. In order at will to have either dry or moist air, tubes provided with stopcocks, and containing in one case chloride of calcium and in the other fragments of glass moistened with water, were inserted between the bellows and the caoutchouc

tube: the air could pass also through a third communication direct to the tube *gg* without being either dried or saturated with moisture. In order to retain a constant temperature, the chloride-of-calcium tube, and the tube containing fragments of glass moistened with water, and also the tube forming the direct communication, were immersed in water contained in a large vessel.

In order that the air blown in might have accurately the same temperature as the pile, it was at the beginning of an experiment led through a long spirally bent tube of copper, which was placed in a large vessel filled with water having the temperature of the pile. This was not easily attainable, and it turned out to be requisite to have the whole room in which the experiments were carried on as nearly as possible at the same temperature as the pile. At the time of the year when it is necessary to heat a room, the heating takes place in the evening; and the morning, when the temperature had equalized itself, was employed for the experiments. It was then a superfluous precaution to place the chloride-of-calcium tube and the tube containing fragments of glass in the same vessel of water.

The thermo-pile, which by preference was taken for this investigation, contained fifty-six pairs of antimony and bismuth plates, which were so thin that a section of the pile did not amount to more than 13 millims. square. In addition to this pile, another was used consisting of twenty-eight pairs in which each bar was 3 millims. in diameter. It displayed effects altogether similar to the more delicate one. Finally, it should be mentioned that the ends of both piles were not pointed, but flat, and lay pretty nearly in the same plane, so that the plates, if they were not in contact with all, were still in contact with the greater number of the bars.

For the observation of the rise of temperature or of the current induced by it, I availed myself of the galvanometer, which is described in the paper "On the Diathermancy of Dry and Moist Air," with two mirrors connected after the manner of astatic needles\*. A very sensitive astatic needle-galvanometer which I had employed in my former researches was, however, sometimes used. In isolated cases both galvanometers, one behind the other, were introduced into the circuit. Then a deflection of one degree of the needle-galvanometer corresponded to 12 millims. on the scale of the mirror-galvanometer—whence the extraordinary sensitiveness of the latter may be inferred. On blowing the air directly against the pile without drying or saturating with moisture, and without covering the pile with the plate, there was no alteration in the galvanometer, provided that

\* Pogg. *Ann.* vol. cxviii. p. 575. *Phil. Mag.* S. 4. vol. xxvi. p. 21.



the temperature of the pile and of the air was the same. This experiment was therefore always available to ascertain whether this equality of temperature was present or not. If the air of the room, previously very far off saturation, were then saturated with moisture, so great an elevation of temperature resulted, that (the pile not being covered with a plate) the deflection of the mirror could no longer be observed, since the image of the scale vanished out of the field of the telescope. To an equal extent, but in an opposite direction, the deflection took place when the air of the room, previously moist, was dried and then admitted to the pile.

When the pile was shielded by a plate, the deflection of the galvanometer was less; and on employing different plates, the deflection was of various extent, according to the nature of the plates, their thickness, and their extent. But it was visible with all the plates which I could try, both with rough and with smooth surfaces, and even when they were coated with varnish. In the instance of a brass plate of 2.5 millims. in thickness, the galvanometer gave a stroke of more than fifty divisions of the scale when moist air was blown against the plate. On continuing uninterruptedly to blow moist or dry air, the galvanometer very gradually turned back to its position indicating equilibrium. The temperature of the room being low, the employment of moist air occasioned only a gradual elevation of the temperature of the plate, doubtless because the air contained only little moisture. If, however, after a certain deflection of the galvanometer had been reached dry air were blown in, then the galvanometer struck quickly towards the opposite side, and went, in consequence of the sudden cooling, far away beyond equilibrium.

Glass plates acted similarly to metallic plates. Also in this case the deflection varied with their thickness and extent. The deflection was particularly marked when a thin glass plate was employed, such as is used for the polarization of light, or for the covering of microscopical objects. Plates of quartz, of gypsum, of mica, of rock-salt, of alum, showed the same phenomenon.

Plates of wood, paste, and different other organic bodies behaved in the same way, as likewise caoutchouc, both vulcanized and non-vulcanized, gutta percha, leather both greased and ungreased, ivory, &c. There was a deflection of the galvanometer of 100 divisions of the scale and more when moist air was blown against plates of paraffin, stearic acid, and wax, of about 2.5 millims. in thickness.

By employing leather which was not greased, pasteboard, thin wood, ivory, gutta percha, and some other substances, the deflection of the galvanometer was at least as great (and sometimes even greater) as in the instance in which dry or moist air was blown immediately against the pile. This arises undoubtedly

from the circumstance that these substances condense aqueous vapours in far larger quantity than the surface of the pile, and that they therefore become more heated or more cooled than that; and hence their action on the pile is as great—or, according to their thickness, greater than the direct action of the vapours when they are immediately absorbed by the pile.

The thickness of the plates must not exceed a certain measure if the alteration of temperature is to be appreciable; but experiments establish the conclusion, *that all substances, however different they may be, are raised in temperature when air comes in contact with them which is moister than that surrounding them, and that they are depressed in temperature when they are exposed to air which is drier than that by which they are surrounded.*

The very considerable heating or cooling which occurs on altering the state of moisture of the atmosphere, led me to suspect that this might possibly be appreciable by an air-thermometer. Such an instrument was therefore used, consisting of two glass bulbs connected by a narrow tube AB (Plate III. fig. 2). Each of the bulbs was protected from accidental cooling by being surrounded by a small glass shade, C and D; and into the tubulure of each shade a glass tube was inserted, through which the air might be blown in. The shades were closed below with cork plates *kk*, which consisted of two pieces, and left a space about the narrow tube of the air-thermometer for the escape of air which had been blown in. On blowing the air of the room into one of these small shades, no alteration in the height of the thermometer took place. If the air had been previously dried, then the bulb operated upon fell in temperature; and if the air had been saturated with aqueous vapour, the bulb rose in temperature, so that the difference of level of the liquid in the narrow tube separating the bulbs amounted to from 4 to 6 millims. If the blowing in of either one or the other kind of air were continued, the fluid assumed gradually its initial level. When, one of the bulbs being coated with pine-soot, alternately a stream of dry and then moist air was blown in upon it, a difference of level of from 8 to 10 millims. was occasioned.

A similar effect took place when alternately dry and moist air was blown against the bulb of a mercurial thermometer divided into half degrees. The thermometer was guarded against currents of air which might interfere, by being placed in a T-shaped tube, being retained in its place by means of a cork (fig. 3). The air was blown in at B. The difference in the height of the thermometer *xy*, produced by the employment of dry and moist air, was from  $0^{\circ}2$  to  $0^{\circ}3$  C. On blackening the bulb of the thermometer, the difference rose to  $0^{\circ}6$  C.

These experiments show how considerable the heating and

cooling at the surface of bodies must be, but they do not enable us to measure it ; for the alteration of temperature must be variable, depending upon the thickness or extent of the body at the surface of which the absorption takes place, and upon the velocity with which heat traverses it. This velocity seems to be very great in all substances, since the alterations of temperature which took place at the surface of the plate, and which could be only very small, were visible on the scale after the lapse of a very short time.

In the instance of rock-salt and other diathermanous substances, the heating must begin immediately. But with metal plates, in which heat travels only by conduction, the shortness of the time in which the action takes place is very striking, and still more so when wood and other bad conductors are employed. It seemed, therefore, desirable to institute some researches which might furnish data concerning this velocity, and thereby at the same time give certainty that no circumstance remained unnoticed which might have contributed towards the results mentioned in the above experiments.

In these researches a cubical box, *HK* (Pl. III. fig. 4), consisting of wood and with sides 30 centims. in extent, was used. In one of the sides of the box there was a circular opening, *ss*, of 10 centims. diameter. The plate *pp*, in which the passage of heat was to be observed, was fastened in front of this opening. The plate was pressed against the side of the box by means of screws or other suitable means. In the box there was a thermo-pile, of which one side was closed by means of its brass cap *i*. The other side, which was uncovered, was pressed against the plate fastened to the box, and, moreover, was placed in the middle of the circular opening ; the pressure was managed by means of a spiral spring, just as in the other instance given above, where the pressure was exercised in a vertical direction.

The wires which connected the pile with the galvanometer came out through small openings in the box, which was otherwise quite closed. Outside of the box, at a distance of 60 millims. from it, there was placed the flame of a lamp, so arranged in relation to the plate to be examined that it was situated in the prolongation of the axis of the thermo-pile.

The cubical box and the flame were enclosed in a great paste-board box, which, to avoid catching fire, was provided with a hole perpendicularly over the flame. By means of this case, all disturbance which might arise from accidental motion of the air of the room was obviated. After the temperature of the pile had become constant, and consequently the indication of the galvanometer constant also, the candle was lit through a hole in the paper box, which could be closed by a curtain, and the deflection of the galvanometer observed with the telescope.



A plate of copper 1·5 millim. in thickness was first used. Immediately on lighting the candle, after the lapse of an interval of time which was hardly measurable, the deflection began. Whereupon many such plates were employed, being placed one close to the other up to eight, and which together formed a layer of 12 millims. in thickness. The deflection began in about one minute. After ten minutes the light was extinguished. At this time the deflection amounted to from 150 to 200 divisions of the scale. Thereupon this increased still during the following ten minutes from 100 to 200 divisions further, and then began to decline. When afterwards the plates, separated from one another, were disposed as is figured (fig. 4), whereby their distances from middle to middle of the plate amounted to 15 millims., and the light stood at 25 millims. from the outermost plate, then the deflection commenced in little more than one minute after lighting the candle. Yet in this instance, after the lapse of ten minutes after extinguishing the candle, it amounted to 40 or 50 divisions of the scale. After this it increased gradually for half an hour, until it reached about 30 divisions more.

Plates of wood behaved just like the copper plates. On taking a board of beechwood of 27 millims. in thickness, the deflection of the galvanometer began in two minutes after the lighting of the candle. In nine minutes it amounted to 750 divisions of the scale. On putting out the light, the deflection increased yet during the next three minutes about 40 divisions, and then became so great that the scale did not admit of further observation of it. After an hour the deflection was still 200 divisions.

Even through a beechen board of 65 millims. in thickness the propagation of heat was still very plainly to be made out. The deflection began eight minutes after lighting the candle, and attained in ten minutes after the light had been extinguished 28 divisions on the scale, and still continued to increase until, after the lapse of half an hour, it had attained to 290 divisions.

Not alone with the thermo-pile, but also with the above-described air-thermometer could the passage of heat through many plates be perceived. In a large box of pasteboard there was a partition with an opening in it, and before the opening two very thin plates of brass-foil were fastened at a distance of 15 millims. from one another. On one side of the same the light was fastened at a distance of 60 millims. from the nearest plate. On the other was placed the air-thermometer, of which the blackened bulb was as near as possible to the other plate without actually touching it. The height of the thermometer was read off through an opening in the box by means of a telescope. Shortly after lighting the candle the blackened bulb began to rise in temperature, whereby an alteration of about 4

millims. in the level of the liquid was occasioned. It results from this, that the double shades of Melloni, which are so much used, afford a sufficient protection only at low temperatures, such as occur in the common thermo-electrical experiments; for if in such experiments the source of heat has sometimes a tolerably high temperature, still it operates from so great a distance that the heat which passes from it to the shade is always minute.

In the analytical theory of heat, the alteration of temperature of a point situated at a distance from a constant source of heat is regarded as a function of the distance and of the time elapsed since the presentation of the source of heat. Also the means are given for estimating this heating numerically; these estimations are, however, only relative to the heating which would take place in another substance under similar circumstances. The absolute measurement of the time which is requisite for the attainment of a certain temperature in a point does not enter into the account. After the publication of his *Théorie de la Chaleur*, Fourier proposed to measure the conductivity of thin plates by placing them on a bed of constant temperature, and laying his contact thermometer (consisting of a mass of mercury in which there is a very delicate thermometer), after it had been heated, upon this plate, and noting the times which must elapse in order that the thermometer, by the employment of different plates, may become cooled to a like difference of temperature.

Here the times, which are taken up by each cooling, are indeed directly observed; but Fourier gives no information about the duration of these times in the few experiments which he has made, and, except himself, so far as I know, no one has employed his method.

Moreover the theory of the propagation of heat will gain nothing from the above-described experiments. Nor are they adapted for the measurement of the times which elapse before different plates undergo a given alteration of temperature; for, to this end, the degree of sensitiveness of the thermoscope should be quite unalterable; and that is hardly attainable. Moreover, various other difficulties stand in the way of such admeasurements. But notwithstanding that one cannot make accurate measurements, it remains a very remarkable fact that the minute amount of heat radiated by a flame at a distance of 60 millims. from a copper plate of 1.5 millim. in thickness should pass through that plate and through seven other similar plates standing 15 millims apart, and should be appreciable after the lapse of one minute.

Collecting together the results of this investigation, it has been established that the most various organic and inorganic

bodies—wax, paraffin, glass, quartz, mica, gypsum, and the most dissimilar salts, also the metals, whether rough or polished, or even covered with varnish—condense on their surface aqueous vapour from the surrounding air which has the same temperature as themselves, and in consequence of this condensation undergo elevation of temperature; and that when the surrounding air is changed for air containing less moisture, then a part of the previously condensed moisture should evaporate and cool the surface of the body.

Results perfectly similar to those obtained with vapour of water were obtained by using vapour of alcohol, or of ether, or other vapours.

Generalizing, *the most various vapours condense on the surface of solid bodies in such quantity as to cause appreciable elevation of temperature.*

From this it follows *that at all times there is at the surface of solid bodies a layer of condensed vapour, which is larger or smaller according to the hygrometric state of the atmosphere.* Under some conditions this will, without doubt, exercise a by no means unimportant influence.

#### XLIV. *On the Influence of Condensation in Experiments upon Diathermancy.* By G. MAGNUS\*.

**R**ESPECTING the influence which condensation exercises upon the thermo-pile, and from which this investigation sets out, Dr. Tyndall says that it has been known to him for years†. It is to be regretted that in his numerous publications upon the transmission of heat, Dr. Tyndall has nowhere made mention of this important circumstance, and that he has nowhere given the precautions he has taken in order to avoid this disturbing influence when blowing or sucking air through tubes open at both ends. Dr. Tyndall‡ has had his experiments repeated by Dr. Frankland, in order, as he says, to prove that he “had not mistaken cold for hot, and hot for cold.” Such a confirmation was, in my opinion, unnecessary. I have not implied an error of that kind, but have only said that, on repeating the experiments of Dr. Tyndall, it has not even once happened to me to obtain the same result as he did. From that I infer that in these experiments, and in the repetitions by Dr. Frankland, some circumstance has come into play which up to the present has remained unascertained, or at any rate unmentioned. Dr.

\* Translated by Prof. Wanklyn from Poggendorff's *Annalen*, vol. cxxi. part 1 (1864, No. 1).

† Phil. Mag. S. 4. vol. xxvi. p. 45, July 1863.

‡ Ibid. p. 46



Tyndall suggests a different reason to account for my not succeeding in getting the same result as he. This he expresses by means of a comparison\*, viz. that he weighs on a balance which weighs to the milligramme, whilst I on one which can only weigh pounds†.

I must leave it to others to judge how far Dr. Tyndall is right in making such a comparison between his work and mine. I think it is certain that the apparatus which I employed, both the thermo-pile and the galvanometer, were at least as sensitive as his; and my method, to which all the objections raised by him do not apply, I must regard as better, inasmuch as I dispense with the employment of hygroscopic plates of rock-salt, which, even if they remain dry, interfere with the sharpness of the observations, since they let through only a part of the heat which falls upon them. Notwithstanding Dr. Tyndall's protest, I believe that I can deduce with certainty from my experiments that air containing aqueous vapour lets through the rays of heat only a little less readily than air in the dry state.

XLV. *On Reciprocal Figures and Diagrams of Forces.* By J. CLERK MAXWELL, F.R.S., *Professor of Natural Philosophy in King's College, London*‡.

**R**ECIPROCAL figures are such that the properties of the first relative to the second are the same as those of the second relative to the first. Thus inverse figures and polar reciprocals are instances of two different kinds of reciprocity.

The kind of reciprocity which we have here to do with has reference to figures consisting of straight lines joining a system of points, and forming closed rectilinear figures; and it consists in the directions of all lines in the one figure having a constant relation to those of the lines in the other figure which correspond to them.

In plane figures, corresponding lines may be either parallel,

\* Phil. Mag. S. 4. vol. xxvi. p. 53.

† It must seem strange that one cannot tell at the present time how great, according to Dr. Tyndall, is the difference of absorption by dry and by moist air. In the paper, Phil. Mag. S. 4. vol. xxiv. p. 422, he says, in experiments on atmospheric air (p. 426), "Aqueous vapour absorbs in certain cases sixty times as much calorific rays as the air which contains it;" and Phil. Mag. S. 4. vol. xxv. p. 205, atom compared with atom, the absorption of aqueous vapour is 16,000 times as great as that of air. In Phil. Mag. S. 4. vol. xxvi. p. 36, Dr. Tyndall calculates, on the other hand, the absorption of the aqueous vapour in a tube 4 feet long to be 4.2 per cent., or 6 per cent. of the entire rays.

‡ Communicated by the Author.

perpendicular, or at any constant angle. Lines meeting in a point in one figure form a closed polygon in the other.

In figures in space, the lines in one figure are perpendicular to planes in the other, and the planes corresponding to lines which meet in a point form a closed polyhedron.

The conditions of reciprocity may be considered from a purely geometrical point of view; but their chief importance arises from the fact that either of the figures being considered as a system of points acted on by forces along the lines of connexion, the other figure is a diagram of forces, in which these forces are represented in plane figures by lines, and in solid figures by the areas of planes.

The properties of the "triangle" and "polygon" of forces have been long known, and the "diagram" of forces has been used in the case of the funicular polygon; but I am not aware of any more general statement of the method of drawing diagrams of forces before Professor Rankine applied it to frames, roofs, &c. in his 'Applied Mechanics,' p. 137, &c. The "polyhedron of forces," or the equilibrium of forces perpendicular and proportional to the areas of the faces of a polyhedron, has, I believe, been enunciated independently at various times; but the application to a "frame" is given by Professor Rankine in the *Philosophical Magazine*, February 1864.

I propose to treat the question geometrically, as reciprocal figures are subject to certain conditions besides those belonging to diagrams of forces.

### *On Reciprocal Plane Figures.*

*Definition.*—Two plane figures are reciprocal when they consist of an equal number of lines, so that corresponding lines in the two figures are parallel, and corresponding lines which converge to a point in one figure form a closed polygon in the other.

*Note.*—If corresponding lines in the two figures, instead of being parallel are at right angles or any other angle, they may be made parallel by turning one of the figures round in its own plane.

Since every polygon in one figure has three or more sides, every point in the other figure must have three or more lines converging to it; and since every line in the one figure has two and only two extremities to which lines converge, every line in the other figure must belong to two, and only two closed polygons. The simplest plane figure fulfilling these conditions is that formed by the six lines which join four points in pairs. The reciprocal figure consists of six lines parallel respectively to these, the points in the one figure corresponding to triangles in the other.

*General Relation between the Numbers of Points, Lines, and Polygons in Reciprocal Figures.*

The effect of drawing a line, one of whose extremities is a point connected with the system of lines already drawn, is either to introduce one new point into the system, or to complete one new polygon, or to divide a polygon into two parts, according as it is drawn to an isolated point, or a point already connected with the system. Hence the sum of points and polygons in the system is increased by one for every new line. But the simplest figure consists of four points, four polygons, and six lines. Hence the sum of the points and polygons must always exceed the number of lines by two.

*Note.*—This is the same relation which connects the numbers of summits, faces, and edges of polyhedra.

*Conditions of indeterminateness and impossibility in drawing reciprocal Diagrams.*

Taking any line parallel to one of the lines of the figure for a base, every new point is to be determined by the intersection of two new lines. Calling  $s$  the number of points or summits,  $e$  the number of lines or edges, and  $f$  the number of polygons or faces, the assumption of the first line determines two points, and the remaining  $s-2$  points are determined by  $2(s-2)$  lines. Hence if

$$e = 2s - 3,$$

every point may be determined. If  $e$  be less, the form of the figure will be in some respects indeterminate; and if  $e$  be greater, the construction of the figure will be impossible, unless certain conditions among the directions of the lines are fulfilled.

These are the conditions of drawing any diagram in which the directions of the lines are arbitrarily given; but when one diagram is already drawn in which  $e$  is greater than  $2s-3$ , the directions of the lines will not be altogether arbitrary, but will be subject to  $e-(2s-3)$  conditions.

Now if  $e'$ ,  $s'$ ,  $f'$  be the values of  $e$ ,  $s$ , and  $f$  in the reciprocal diagram

$$\begin{aligned} e &= e', & s &= f', & f &= s', \\ e &= s + f - 2, & e' &= s' + f' - 2. \end{aligned}$$

Hence if  $s=f$ ,  $e=2i-2$ ; and there will be one condition connecting the directions of the lines of the original diagram, and this condition will ensure the possibility of constructing the reciprocal diagram. If

$$s > f, \quad e > 2s - 2, \quad \text{and} \quad e' < 2s' - 2;$$

so that the construction of the reciprocal diagram will be possible, but indeterminate to the extent of  $s-f$  variables.



If  $s < f$ , the construction of the reciprocal diagram will be impossible unless  $(s-f)$  conditions be fulfilled in the original diagram.

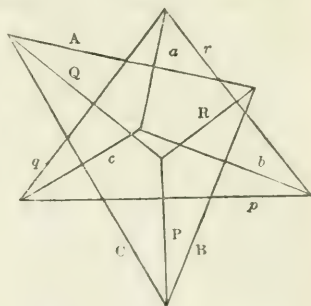
If any number of the points of the figure are so connected among themselves as to form an equal number of closed polygons, the conditions of constructing the reciprocal figure must be found by considering these points separately, and then examining their connexion with the rest.

Let us now consider a few cases of reciprocal figures in detail. The simplest case is that of the figure formed by the six lines connecting four points in a plane. If we now draw the six lines connecting the centres of the four circles which pass through three out of the four points, we shall have a reciprocal figure, the corresponding lines in the two figures being at right angles.

The reciprocal figure formed in this way is definite in size and position; but any figure similar to it and placed in any position is still reciprocal to the original figure. If the reciprocal figures are lettered as in fig. 1, we shall have the relation

$$\frac{AP}{ap} = \frac{BQ}{bq} = \frac{CR}{cr}.$$

Fig. 1.



In figures 2 and II. we have a pair of reciprocal figures in which the lines are more numerous, but the construction very easy. There are seven points in each figure corresponding to seven polygons in the other.

Fig. 2.

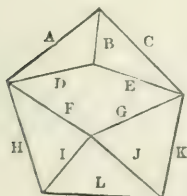
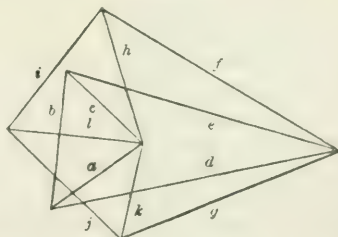


Fig. II.



The four points of triple concurrence of lines ABC, BDE, HIL, LJK correspond to four triangles,  $abc$ ,  $bde$ ,  $hil$ ,  $ljk$ .

The three points of quadruple concurrence ADFH, CEKG, IFGJ correspond to three quadrilaterals,  $adf$ ,  $ceg$ ,  $ifg$ .

The five triangles  $A D B$ ,  $E B C$ ,  $G J K$ ,  $I J L$ ,  $H I F$  correspond to five points of triple concurrence,  $adb$ ,  $ebc$ ,  $gjk$ ,  $ijl$ ,  $hif$ .

The quadrilateral  $D E G F$  corresponds to the point of quadruple concurrence  $degf$ .

The pentagon  $A C K L H$  corresponds to the meeting of the five lines  $acklh$ .

In drawing the reciprocal of fig. 2, it is best to begin with a point of triple concurrence. The reciprocal triangle of this point being drawn, determines three lines of the new figure. If the other extremities of any of the lines meeting in this point are points of triple concurrence, we may in the same way determine more lines, two at a time. In drawing these lines, we have only to remember that those lines which in the first figure form a polygon, start from one point in the reciprocal figure. In this way we may proceed as long as we can always determine all the lines except two of each successive polygon.

The case represented in figs. 3 and III. is an instance of a pair of reciprocal figures fulfilling the conditions of possibility and determinateness, but presenting a slight difficulty in drawing by the foregoing rule. Each figure has here eight points and eight polygons; but after we have drawn the lines  $s$ ,  $n$ ,  $o$ ,  $k$ ,  $r$ , we cannot proceed with the figure simply by drawing the last two lines of polygons, because the next polygons to be drawn are quadrilaterals, and we have only one side of each given. The easiest way to proceed is to produce  $abcd$  till they form a quadrilateral, then to draw a subsidiary figure similar to  $tlmpq$ , with  $abcd$  similarly situated, and then to reduce the latter figure to such a scale and position that  $a$ ,  $b$ ,  $c$ ,  $d$  coincide in both figures.

Fig. 3.

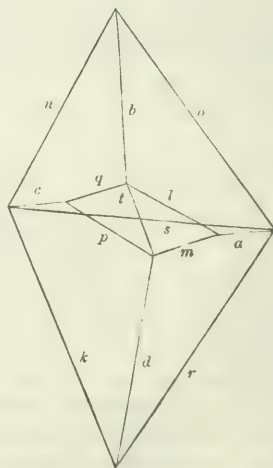
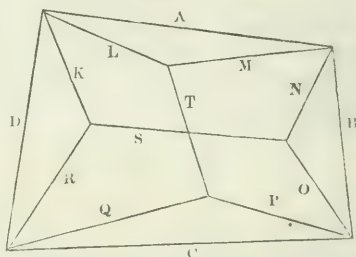


Fig. III.

In figures 4 and IV. the condition that the number of polygons is equal to the number of points is not fulfilled. In fig. 4 there

Fig. 1V.

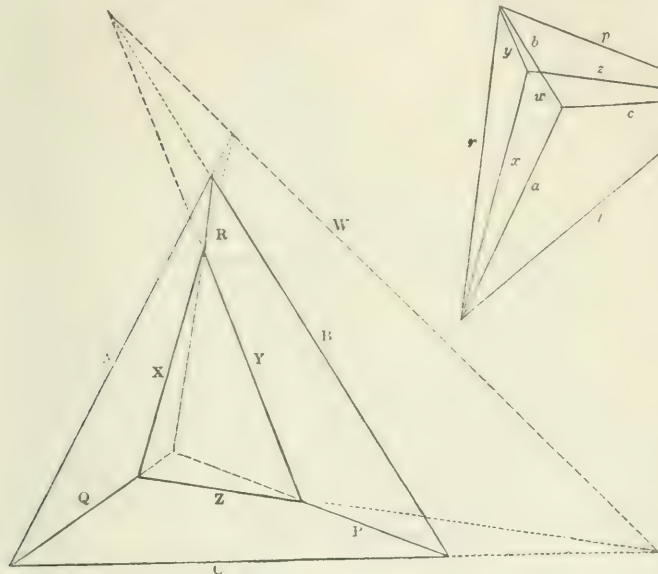
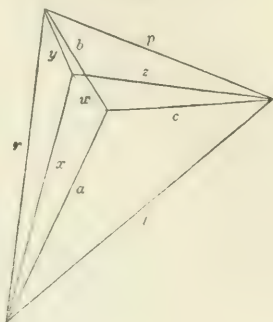


Fig. 4.



are five points and six triangles; in fig. IV. there are six points, two triangles, and three quadrilaterals. Hence if fig. 4 is given, fig. IV. is indeterminate to the extent of one variable, besides the elements of scale and position. In fact when we have drawn  $ABC$  and indicated the directions of  $P, Q, R$ , we may fix on any point of  $P$  as one of the angles of  $XYZ$  and complete the triangle  $XYZ$ . The size of  $XYZ$  is therefore indeterminate. Conversely, if fig. IV. is given, fig. 4 cannot be constructed unless one condition be fulfilled. That condition is that  $P, Q$ , and  $R$  meet in a point. When this is fulfilled, it follows by geometry that the points of concurrence of  $A$  and  $X$ ,  $B$  and  $Y$ , and  $C$  and  $Z$  lie in one straight line  $W$ , which is parallel to  $w$  in fig. 4. The condition may also be expressed by saying that fig. IV. must be a perspective projection of a polyhedron whose quadrilateral faces are planes. The planes of these faces intersect at the concurrence of  $P, Q, R$ , and those of the triangular faces intersect in the line  $W$ .

Figs. 5 and V. represent another case of the same kind. In fig. 5 we have six points and eight triangles; fig. V. is therefore



capable of two degrees of variability, and is subject to two conditions.

Fig. 5.

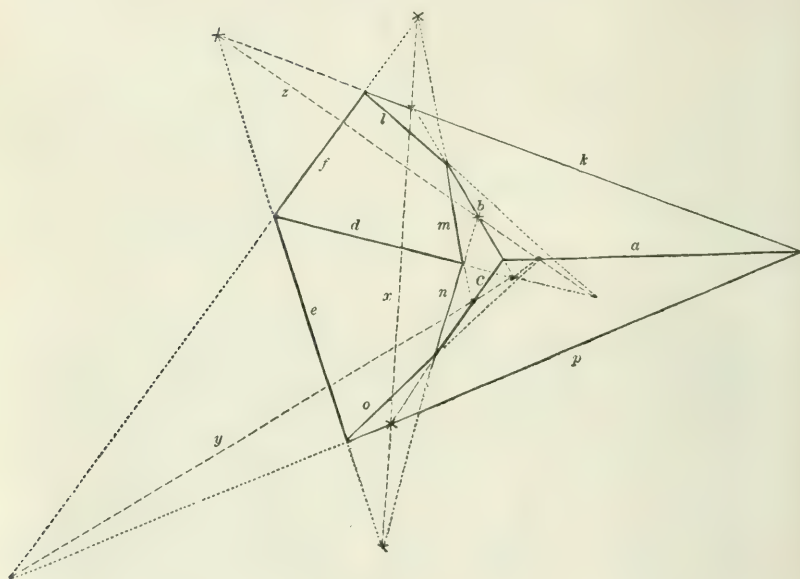
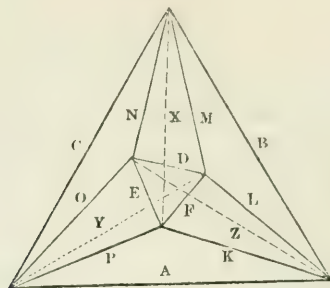


Fig. V.

The conditions are that the four intersections of corresponding sides of opposite quadrilaterals in fig. V. shall lie in one straight line, parallel to the line joining the opposite points of fig. 5 which correspond to these quadrilaterals. There are three such lines marked  $x$ ,  $y$ ,  $z$ , and four points of intersection lie on each line.

We may express this condition also by saying that fig. V. must be a perspective projection of a plane-sided polyhedron, the intersections of opposite planes being the lines  $x$ ,  $y$ ,  $z$ .

In fig. 6, let  $A B C D E$  be a portion of a polygon bounded by other polygons of which the edges are  $P Q R S T$ , one or more of these edges meeting each angle of the polygon.

In fig. VI., let  $abcde$  be lines parallel to  $A B C D E$  and meeting in a point, and let these be terminated by the lines  $p q r s t$  parallel to  $P Q R S T$ , one or more of these lines completing each sector of fig. VII.

In fig. 6 draw  $Y$  through the intersections of  $A C$  and  $P Q$ , and in fig. VI. draw  $y$  through the intersections of  $a, p$  and  $c, q$ . Then the figures of six lines  $A B C P Q Y$  and  $abc p q y$  will be reciprocal, and  $y$  will be parallel to  $Y$ . Draw  $X$  parallel to  $x$ , and through the intersections of  $T X$  and  $C E$  draw  $Z$ , and in fig. VI. draw  $z$  through the intersections of  $c x$  and  $e t$ ; then  $C D E T X Z$  and  $c d e t x z$  will be reciprocal, and  $Z$  will be parallel to  $z$ . Then through the intersections of  $A E$  and  $Y Z$  draw  $W$ , and through those of  $a y$  and  $e z$  draw  $w$ ; and since  $A C E Y Z W$  and  $a c e y z w$  are reciprocal,  $W$  will be parallel to  $w$ .

By going round the remaining sides of the polygon  $A B C D E$  in the same way, we should find by the intersections of lines another point, the line joining which with the intersection of  $A E$  would be parallel to  $w$ , and therefore we should have three points in one line; namely, the intersection of  $Y$  and  $Z$ , the point determined by a similar process carried on on the other part of the circumference of the polygon, and the intersection of  $A$  and  $E$ ; and we should find similar conditions for every pair of sides of every polygon.

Now the conditions of the figure 6 being a perspective projection of a plane-sided polyhedron are exactly the same. For  $A$  being the intersection of the faces  $A P$  and  $A B$ , and  $C$  that of  $B C$  and  $Q C$ , the intersection  $A C$  will be a point in the intersection of the faces  $A P$  and  $C Q$ .

Similarly the intersection  $P Q$  will be another point in it, so that  $Y$  is the line of intersection of the faces  $A P$  and  $C Q$ .

In the same way  $Z$  is the intersection of  $E T$  and  $C Q$ , so that the intersection of  $Y$  and  $Z$  is a point in the intersection of  $A P$  and  $E T$ .

Fig. 6.

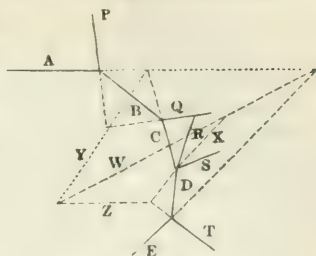
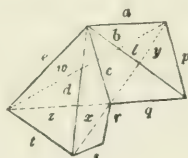


Fig. VI.



Another such point can be determined by going round the remaining sides of the polygon ; and these two points, together with the intersections of the lines A E, must all be in one straight line, namely, the intersection of the faces A P and E T.

Hence the conditions of the possibility of reciprocity in plane figures are the same as those of each figure being the perspective projection of a plane-sided polyhedron. When the number of points is in every part of the figure equal to or less than the number of polygons, this condition is fulfilled of itself. When the number of points exceeds the number of polygons, there will be an impossible case, unless certain conditions are fulfilled so that certain sets of intersections lie in straight lines.

### *Application to Statics.*

The doctrine of reciprocal figures may be treated in a purely geometrical manner, but it may be much more clearly understood by considering it as a method of calculating the forces among a system of points in equilibrium ; for,

If forces represented in magnitude by the lines of a figure be made to act between the extremities of the corresponding lines of the reciprocal figure, then the points of the reciprocal figure will all be in equilibrium under the action of these forces.

For the forces which meet in any point are parallel and proportional to the sides of a polygon in the other figure.

If the points between which the forces are to act are known, the problem of determining the relations among the magnitudes of the forces so as to produce equilibrium will be indeterminate, determinate, or impossible, according as the construction of the reciprocal figure is so.

Reciprocal figures are mechanically reciprocal ; that is, either may be taken as representing a system of points, and the other as representing the magnitudes of the forces acting between them.

In figures like 1, 2 and II., 3 and III., in which the equation

$$e = 2s - 2$$

is true, the forces are determinate in their ratios ; so that one being given, the rest may be found.

When  $e > 2s - 2$ , as in figs. 4 and 5, the forces are indeterminate, so that more than one must be known to determine the rest, or else certain relations among them must be given, such as those arising from the elasticity of the parts of a frame.

When  $e < 2s - 2$ , the determination of the forces is impossible except under certain conditions. Unless these be fulfilled, as in figs. IV. and V., no forces along the lines of the figure can keep its points in equilibrium, and the figure, considered as a frame, may be said to be loose.

When the conditions are fulfilled, the pieces of the frame can support forces, but in such a way that a small disfigurement of the frame may produce infinitely great forces in some of the pieces, or may throw the frame into a loose condition at once.

The conditions, however, of the possibility of determining the ratios of the forces in a frame are not coextensive with those of finding a figure perfectly reciprocal to the frame. The condition of determinate forces is  $e = 2s - 2$ ;

the condition of reciprocal figures is that every line belongs to two polygons only, and  $e = s + f - 2$ .

In fig. 7 we have six points connected by ten lines in such a way that the forces are all determinate; but since the line *L* is a side of three triangles, we cannot draw a reciprocal figure, for we should have to draw a straight line *l* with three ends.

If we attempt to draw the reciprocal figure as in fig. VII., we shall find that, in order to represent the reciprocals of all the lines of fig. 7 and fix their relations, we must repeat two of them, as *h* and *e* by *h'* and *e*, so as to form a parallelogram. Fig. VII. is then a complete representation of the relations of the force which would produce equilibrium in fig. 7; but it is redundant by the repetition of *h* and *e*, and the two figures are not reciprocal.

Fig. 7.

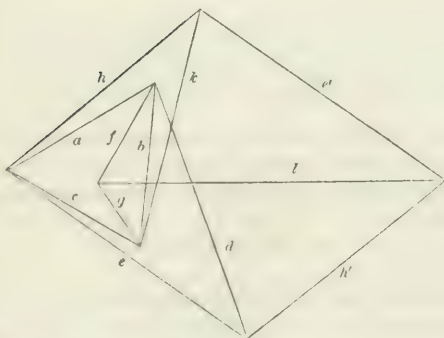
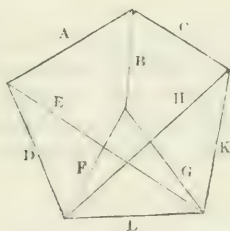


Fig. VII.



*On Reciprocal Figures in three dimensions.*

*Definition.*—Figures in three dimensions are reciprocal when they can be so placed that every line in the one figure is perpendicular to a plane face of the other, and every point of concurrence of lines in the one figure is represented by a closed polyhedron with plane faces.

The simplest case is that of five points in space with their ten connecting lines, forming ten triangular faces enclosing five tetrahedrons. By joining the five points which are the centres of the spheres circumscribing these five tetrahedrons, we have a reciprocal figure of the kind described by Professor Rankine in the *Philosophical Magazine*, February 1864; and forces proportional to the areas of the triangles of one figure, if applied along the corresponding lines of connexion of the other figure, will keep its points in equilibrium.

In order to have perfect reciprocity between two figures, each figure must be made up of a number of closed polyhedra having plane faces of separation, and such that each face belongs to two and only two polyhedra, corresponding to the extremities of the reciprocal line in the other figure. Every line in the figure is the intersection of three or more plane faces, because the plane face in the reciprocal figure is bounded by three or more straight lines.

Let  $s$  be the number of points or summits,  $e$  the number of lines or edges,  $f$  the number of faces, and  $c$  the number of polyhedra or cells. Then if about one of the summits in which polyhedra meet, and  $\sigma$  edges and  $\eta$  faces, we describe a polyhedral cell, it will have  $\phi$  faces and  $\sigma$  summits and  $\eta$  edges, and we shall have

$$\eta = \phi + \sigma - 2;$$

$s$ , the number of summits, will be decreased by one and increased by  $\sigma$ ;

$c$ , the number of cells, will be increased by one;

$f$ , the number of faces, will be increased by  $\phi$ ;

$e$ , the number of edges, will be increased by  $\eta$ ;

so that  $e + c - (s + f)$  will be increased by  $\eta + 1 - (\sigma + \phi - 1)$ , which is zero, or this quantity is constant. Now in the figure of five points already discussed,  $e = 10$ ,  $c = 5$ ,  $s = 5$ ,  $f = 10$ ; so that generally

$$e + c = s + f,$$

in figures made up of cells in the way described.

The condition of a reciprocal figure being indeterminate, de-

terminate, or impossible except in particular cases, is

$$e \begin{matrix} > \\ = \\ < \end{matrix} 3s - 5.$$

This condition is sufficient to determine the possibility of finding a system of forces along the edges which will keep the summits in equilibrium; but it is manifest that the mechanical problem may be solved, though the reciprocal figure cannot be constructed owing to the condition of all the sides of a face lying in a plane not being fulfilled, or owing to a face belonging to more than two cells. Hence the mechanical interest of reciprocal figures in space rapidly diminishes with their complexity.

Diagrams of forces in which the forces are represented by lines may be always constructed in space as well as in a plane, but in general some of the lines must be repeated.

Thus in the figure of five points, each point is the meeting place of four lines. The forces in these lines may be represented by five *gauche* quadrilaterals (that is, quadrilaterals not in one plane); and one of these being chosen, the other four may be applied to its sides and to each other so as to form five sides of a *gauche* hexahedron. The sixth side, that opposite the original quadrilateral, will be a parallelogram, the opposite sides of which are repetitions of the same line.

We have thus a complete but redundant diagram of forces consisting of eight points joined by twelve lines, two pairs of the lines being repetitions. This is a more convenient though less elegant construction of a diagram of forces, and it never becomes geometrically impossible as long as the problem is mechanically possible, however complicated the original figure may be.

XLVI. *On the Difference between Active and Ordinary Oxygen.*

By R. CLAUSIUS\*.

**I**N a paper communicated in March 1858, "On the Nature of Ozone"†, I gave an explanation of this modification of oxygen, which stood in connexion with the views I had shortly before published as to the internal condition of bodies, especially of gases. Our knowledge of ozone was at that time much less than now. The antithesis between ozone and antozone had not been discovered. It was merely known that oxygen by various processes could be brought into an excited condition, in which its oxidizing action is more energetic than that of ordinary oxygen;

\* Translated from vol. viii. of the *Vierteljahrsschrift der naturforschenden Gesellschaft in Zürich*. Read October 18, 1863.

† Phil. Mag. S. 4. vol. xvi. p. 45.

and the oxygen thus altered was called ozone. But, together with an oxidizing action, a deoxidizing action was also then observed for the first time, the deoxidation of binoxide of lead; and it was the publication of this observation\* that led to the communication of my views on ozone.

Since then numerous and important experimental observations on ozone have been made. Schönbein himself has completed his very important discovery of ozone by proving the difference between ozone and antozone. Among other researches, I must specially mention the highly interesting publication of Meissner, *Untersuchungen über den Sauerstoff*; further, the continued investigation of Andrews and Tait†, and the beautiful observations made by Babo‡ and Soret§.

The results of these newer investigations have in a surprising manner confirmed the chief part of my explanation; but in two points, of inferior importance however, they do not quite agree with it. These deviations may perhaps with many readers raise a doubt as to the accuracy of my explanation, as it is not always easy to distinguish the essential from the non-essential of an explanation; and this distinction has been made the more difficult in the present case because, in my first explanation, I laid greater weight upon a non-essential point than was necessary. I consider it convenient, therefore, once more to revert to the subject, in order to state in how far I at present consider my explanation correct, and in what points, on the other hand, in my view, small alterations have become necessary in consequence of the new investigations.

In my paper "On the Nature of the Motion which we call Heat"||, I have drawn the conclusion that in ordinary oxygen the atoms are not separated, but are joined in pairs forming molecules,—a conclusion which also agrees with the views enunciated by Gerhardt "On the Constitution of the Molecules of Gas," excepting that Gerhardt has spoken less definitely about oxygen than I; for he only says¶, "the free oxygen-atom is composed of several (at least two) atoms." Resting on this conclusion, which I had previously drawn, I explained the active oxygen contained in ordinary oxygen, which was then called ozone simply, by saying that it consisted of individual atoms not united in pairs forming molecules, which are dispersed among the ordinary molecules.

\* Phil. Mag. January 1858, page 24. † Phil. Trans., 1860, 113.

‡ *Berichte der naturf. Gesellschaft zu Freiburg im Breisgau*, vol. iii. part 1.

§ *Comptes Rendus*, vol. lvii. p. 604.

|| *Pogg. Ann.* vol. c. p. 353. Phil. Mag. S. 4. vol. xiv. p. 108.

¶ Gerhardt, *Traité de Chimie*, vol. iv. p. 574.

Comparing this explanation with the then known facts, and considering first the most important of the methods for preparing ozone, I had occasion to enter upon the condition of the two atoms contained in an ordinary oxygen-molecule, and to express my opinion on the subject. I said that the process which takes place when ozone is produced by placing atmospheric air in contact with moist phosphorus, may be conceived to take place in the following manner\*:—By the combination of the phosphorus with the surrounding oxygen, a number of the molecules coming in contact with it must be decomposed into their two atoms; and it may thus happen that it does not unite with both, but that one is removed by the motion of heat from its sphere of operation and then remains isolated. It is possible that another circumstance operates also in this case. It is known from electrolysis that, in the union of dissimilar atoms to form one molecule, one part of the molecule becomes positively, and the other negatively electrical. This perhaps takes place also when two similar atoms unite—for example, two oxygen-atoms—by one of these becoming positive and the other negative. But since, in the oxidation of phosphorus, oxygen certainly enters as negative constituent into the compound, it may be that of the two oxygen-atoms resulting from one molecule, the negative is preferably retained by the phosphorus, and the positive, being unrestrained, or at all events less restrained, can escape.

In these sentences, and repeatedly throughout the paper, the view has been expressed—as far as I know, for the first time, and at a time when there were no experimental data which necessitated it—that the atoms in the ordinary oxygen-molecules are in opposite electrical conditions. By the discovery made shortly after, that there are two kinds of active oxygen, which Schönbein has called ozone and autozone, and that these two can unite to form ordinary oxygen, this view has been remarkably confirmed.

With regard to the circumstance that active oxygen can exert an oxidizing and also a deoxidizing action, I gave the following explanation. Isolated atoms can form compounds with other bodies more easily than such as are already joined in pairs, and must first be liberated from this union before they are fitted for combination with other bodies; the former will therefore exert a stronger oxidizing action than the latter. Suppose, further, that an oxide, for example a peroxide, which readily gives up its oxygen or a part of it, is in contact with a gas in which there are oxygen-atoms that have the tendency to combine with second atoms; these will be able to take from the oxide the feebly united

\* *Phil. Mag.* S. 4. vol. xvi. p. 47.



atoms, by which the oxide is reduced and at the same time the active oxygen is transformed into ordinary oxygen.

With reference to this double action of oxidation and deoxidation, I compared active oxygen as it may be contained in ordinary oxygen, with that oxygen which is loosely united in certain peroxides, or in the oxides of the noble metals; and taking peroxide of hydrogen as an example, I said\*, "Peroxide of hydrogen, for instance, has a strong oxidizing action, for it readily gives up its second oxygen-atom. If, on the contrary, peroxide of hydrogen is brought in contact with the oxides of the noble metals, or with certain metallic peroxides, a mutual reduction takes place. Here it may be assumed that the oxygen-atoms which separate from peroxide of hydrogen unite to form molecules with those which are liberated from the metallic oxides or peroxides."

I then proposed the question, Why cannot the readily separable oxygen-atoms contained in an individual oxide or peroxide unite with each other as readily as the oxygen-atoms of one compound unite with the oxygen-atoms of another? Among the reasons which I considered possible for answering this question occurs this, that the oxygen-atoms of different compounds may be in different electrical conditions, and the electrical difference may make the atoms of one compound more suitable for uniting with the atoms of another compound than among themselves.

On the mutual reduction of two peroxides, Brodie, in a beautiful paper published in the Philosophical Transactions for 1850, which was unknown to me at the time I wrote my paper, has expressed an opinion which in one point resembles my own, but in others is essentially different. Brodie assumes that the oxygen of the two compounds which act on one another are in different chemical conditions. He says, oxygen is "chemically polar" in the compounds, and he distinguishes the positive polar and the negative polar condition. Two quantities of oxygen in these two conditions seek to unite chemically just as oxygen and hydrogen can unite. The question, upon what does the chemical difference of both quantities of oxygen depend? and as to how the molecules are constituted, he does not decide, but at the end of his paper states that this question is an open one. He seems, however, inclined to the view that the substances which in chemistry are considered simple, may even be composed of other elements, "that they consist of yet other and further elements." Of oxygen he says specially, "On this view the real fact which lay hid under these phenomena might be the synthesis of the oxygen from the ultimate and further elements of which the oxygen consisted."

\* Phil. Mag. S. 4. vol. xvi. p. 50.

My explanation, on the contrary, definitely refers the phenomena to a simple molecular constitution; for it starts by assuming that the molecule of ordinary oxygen is diatomic, and that the atoms have the tendency, when they are free, again to combine in pairs to form molecules. When there is an electrical opposition between two oxygen-atoms, their union will thereby be promoted; but even when this opposition does not exist, the tendency to union is still present, and the electrical opposition is then spontaneously produced by their union.

In this manner it is explained why the oxygen-atoms of a compound unite more readily with the oxygen-atoms of another compound which are in another electrical state, but that, under suitable conditions (for instance, with increased temperature), the oxygen-atoms of an individual compound may leave it and unite to form molecules, and that thereby oxygen is formed of the same kind as when two quantities of oxygen contained in different compounds come together. Brodie's view, according to which only such quantities of oxygen as have opposite chemical polarity seek to unite, leaves this last process unexplained; and even also in the other processes there is a greater indefiniteness than in my explanation.

From what I have said, I can express so much of the explanation contained in my former paper as, after recent discoveries, I conceive myself justified in adhering to, in the following two sentences, one of which formed from the first the chief point of my explanation, and the other in the course of the discussion was taken as probably true:—

1. *Ordinary oxygen consists of united, active oxygen of isolated atoms.*
2. *The two atoms which form a molecule of ordinary oxygen are in opposite electrical conditions.*

I proceed now to discuss the points in which I consider I must alter the view which I originally expressed.

At that time, as already mentioned, it was not known that there was, besides ozone, a second kind of active oxygen; and with regard to ozone, it was not known that in its relation to the oxygen contained in various compounds there was any such difference as prevails in the deportment of one kind of electricity to the same or to the opposite electricity. Hence I believed that I must consider it as a fact that such a difference does not exist. Now, on the one hand, according to my view of the condition of the ordinary oxygen-molecules, it was presupposed that the atoms of a molecule at the moment in which they separate are of opposite electricities; but, on the other, I considered it as a fact established by experiment that the active oxygen resulting from this separation exhibited no properties

in its subsequent existence which correspond to this electrical antithesis, and hence I assumed that the electro-positive or electro-negative condition which the atoms have at the moment of separation are afterwards lost, and the atoms become electrically neutral. It will, however, be admitted that this assumption was not necessarily implied in the idea at the basis of my explanation, but that it was only a collateral assumption, which was resorted to in order to satisfy the then known state of things. Hence, when required by our improved knowledge, it can be given up and changed without touching the fundamental idea of my explanation.

In chemistry the cases are frequent in which a given quantity of oxygen completely unites with another substance, and in such a manner that all atoms of this oxygen are contained in the same manner, and must hence all have one and the same electrical condition, in most cases the electro-negative. Now if, according to the above assumption, in the oxygen before it enters into combination, half the atoms are positively, and the other half negatively electrical, when the compound is formed half the atoms must change their electrical condition. Inversely, cases occur in which oxygen is separated from a compound, and in which at the moment of separation, as may be assumed, all atoms have the same electrical conditions; while afterwards, when the liberated oxygen has passed into its ordinary condition, half the atoms, according to the assumption, are positive and half negative. According to this, the electrical difference of the oxygen-atoms must not be so conceived as if there were two kinds of oxygen-atoms, of which, once for all, one kind are electro-positive and the other kind electro-negative; but the possibility of a passage from one condition to the other must be conceded.

Herein the possibility is implied that the atoms, momentarily at all events, may be in intermediate conditions, and, among others, may be unelectrical. But whether the transition always takes place suddenly, or whether the atoms can remain in these intermediate conditions for a long time and gradually pass from one to the other, is not thereby decided, and can only be concluded by observation.

With reference to ozone in the restricted sense, the facts recently observed by Schönbein seem to show that the active atoms which form ozone are electro-negative, and retain this condition unchanged as long as ozone, as such, exists. But what the condition of antiozone is in reference to the permanence of its electrical condition, cannot with certainty be concluded from the facts hitherto known.

The second point, in which I consider I must somewhat alter



the explanation I originally gave, is connected with the changes in volume which oxygen undergoes when part of it passes from the ordinary into the active condition.

In the paper above cited, "On the Nature of the Motion which we term Heat," I have referred all volumetric relations of gaseous bodies to one theorem, "that for the same temperature the individual molecules of all gases have the same *vis viva* as regards their progressive motion." If this theorem is correct in the case of all gases, there must for the same temperature and the same pressure be the same number of molecules in the same volume. If we consider a certain quantity of ordinary oxygen, according to my view the atoms are united in pairs forming molecules. If, by the excitation of this oxygen, a number of molecules are decomposed into their atoms, it is a question how these atoms are constituted—whether they remain isolated and effect their motions separately, so that each of these atoms in the gas plays the part of a molecule, or whether they enter into any other combination.

In my first explanation I have assumed that the separated atoms remain separate and form molecules of themselves, so that in excited oxygen there are more molecules than in the same quantity of oxygen in the unexcited condition. There were indeed at that time the experiments of Andrews and Tait\* on the density of ozone, which had given the result opposed to that assumption, that oxygen containing ozone, when converted into ordinary oxygen, increases in volume; but these experiments were at that time so isolated, and from their difficulty seemed to contain so many possible sources of error, that, without doubting the skill and care of these experimenters, I believed I must raise an objection to the reliability of the results, and retain my assumption.

Since then the same gentlemen have continued their investigation of the subject, and Babo and Soret have made some observations on the same point. By these investigations, in the description of which the authors in question only speak of ozone, and not of two kinds of active oxygen, the result previously found, that ozonized oxygen fills a smaller volume than the same quantity of oxygen which is completely in the condition of ordinary oxygen, has become completely confirmed; and the special result has been obtained that the difference between the two volumes is just as great as if that part of oxygen which was in the condition of ozone did not exist.

It becomes now the question whether, and in what manner, my explanation, that active oxygen is distinguished from ordinary oxygen in consisting of isolated molecules, can be made to har-

\* Proceedings of the Royal Society, vol. viii. p. 498.



monize with this fact in reference to the volume. According to these observations, it must be considered as established that the isolated atoms of which ozone consists do not remain isolated and form molecules for themselves, but in some manner or other fix themselves on to the molecules of the surrounding ordinary oxygen, and form with them complicated molecules. But in order to retain the essential part of my explanation, definite assumptions must be made as to the constitution of the complicated molecule thus formed.

It must first be assumed that the complicated molecules do not consist of several pairs of atoms, as if several ordinary oxygen-molecules were united together, but that the atoms which form active oxygen are contained as *unpaired* atoms in the molecules. The simplest case of the kind is that when each of the complicated molecules consists of a pair of atoms, and an active atom joined to it; but if there are several active atoms in it, these must be in such positions that they form among themselves no pairs, but adhere to the molecules as atoms, and as such can be separated from it. In order further to explain the strong oxidizing action of active oxygen, it must be assumed that it is easier to separate those unpaired atoms from the molecules than to separate from one another two atoms forming a pair—that, compared with the force with which two paired atoms mutually adhere, the unpaired atoms are only loosely united.

According to this, the second change which I consider I must make in my explanation consists simply, instead of assuming that the atoms are completely free, in saying that they may possibly be *either free or loosely united*.

The case in which an atom is loosely united to any molecule is chemically very little different from that in which it is free; and it was the more obvious to consider it also in my first explanation, since I even then compared the active oxygen contained in pure oxygen with that which occurs loosely united in the peroxides or oxides of the noble metals. I must therefore confess a want of care on my part in laying any weight upon the point that the atoms are quite free, and for this reason, holding the result of the first observations of Andrews and Tait improbable, and that I did not from the first designate both cases as equally possible. Assuming the alternative, that the unpaired atoms may either be free or loosely united, the explanation includes not only the active oxygen contained in pure oxygen, and that which in any chemical compound is contained in such a manner that it readily passes into other compounds, and thus can so far be called active, but it also includes oxygen *in statu nascente*.

I will make some observations as to how, on my view, the com-

plicated molecules in pure oxygen which contain the active oxygen may be supposed to be constituted. But I must here expressly mention that I do not consider that which can be said upon this point as necessary for my explanation, but think that the explanation so far as it is contained in the preceding may be taken even where different opinions are held as to the specialties of the molecular constitution. Hence before I pass to these observations, I will once more briefly summarize the essential part of my explanation in a form corresponding to the recent discoveries. *The molecules of ordinary oxygen are diatomic, and each contains an electro-positive and an electro-negative atom. Active oxygen consists of unpaired atoms, which may either be free or loosely united; and according as the atoms are electro-positive or electro-negative, they form antiozone or ozone.*

All the above-mentioned observers, who have found that ozonized oxygen occupies a smaller volume than ordinary oxygen, are agreed that in the first the molecules must be more complex than in the latter. This is in fact, as already said, to be regarded as a direct result of those observations, if we consider as established the law that the volume of a gas is proportional to the number of its molecules. But regarding the mode in which the composition of the molecule is to be imagined, they are of different opinions.

Andrews and Tait connect their consideration with experiments which they have made with compound gases, especially nitrous oxide and carbonic oxide. When they produced in these gases the same electrical discharges by which they had effected the excitation of oxygen, they observed, as in this case, a diminution of volume, which they explain by assuming that the constituents of the gases in question under the influence of the discharges are partially liberated from their previous compounds and transformed into others which occupy a smaller volume. Founded upon this, they make a supposition that oxygen is not, as now assumed, a simple body, but a chemical compound, whose constituents may also variously unite with one another. This explanation deviates so widely from the generally received view, that, as I think, it can only be entered upon when no other explanation is possible.

Von Babo holds to a view, formerly expressed by Weltzien\*, which is opposed to my explanation, since it tends to consider ordinary oxygen as composed of simple, and ozonized of diatomic molecules; and he promises to develop his reasons in a subsequent paper. I cannot at all favour this idea, since even the comparison of the volume of oxygen with the volumes of its compounds led me, quite independently of ozone, to the assump-

\* *Ann. der Chem. und Pharm.* vol. cxv. p. 128.

tion that oxygen must consist of diatomic molecules ; and I do not see how the actions of ozone, and the mutual union of ozone and antozone to form ordinary oxygen, are to be explained if the molecule of oxygen is presupposed to be monatomic. Before I can further proceed to a discussion of this view, I must of course wait to see what reasons von Babo adduces for it.

Soret expresses no defined view as to the mode of the composition of the molecules. He gives it first as a result of observation, that ozone must have molecules of more atoms than ordinary oxygen ; and in mentioning that a great number of chemists and physicists now assume that in ordinary oxygen the molecules are already diatomic, he says that, according to this assumption, more than two molecules must be assigned to the molecules of ozone. He first discusses, as an example, the simplest case, that a molecule consists of three atoms, and then continues, " Il est clair que rien dans les faits connus ne prouve que l'ozone résulte du groupement de 3 atomes plutôt que de 4, 5, &c. ; pour déterminer ce nombre il faudrait connaître la densité de ce corps." In a note he says that, according to the experiments of St.-Claire Deville and Troost, and of Bineau, the density of sulphur vapour near its boiling-point is three times as great as at very high temperatures ; and that there is perhaps an analogy between these two conditions of sulphur and the allotropic conditions of oxygen, in which case such a molecular constitution must be presupposed for ozone that its density is three times as great as that of ordinary oxygen. Hence, if ordinary oxygen is diatomic, the molecules of ozone are hexatomic.

I think now that the conditions above adduced, following from my explanation, which the complex molecules in ordinary oxygen must fulfil, if they do not give bases for deciding with certainty on the composition of the molecule, at any rate enable us to draw certain conclusions as to the degree of probability of the different possible constitutions. I will first of all consider ozone in the restricted sense, which from its chemical and physical deportment enables us to conclude that it consists of electro-negative atoms. But since, according to my explanation, the active atoms are contained as isolated atoms in the molecules, and as, further, they must in the present case have the same electrical conditions, it becomes more probable from these reasons that in one molecule there is only one atom of ozone, than that there are several. The case adduced as an example by Soret, in which the complex molecule consists of 3 atoms, appears to me not only as valid as that in which they consist of 4, 5, &c. atoms, but seems to me much more probable. As far as regards the other special case adduced by Soret, in which the molecules were to consist of 6 atoms, I can from my point of view only consider it as very improbable.



Assuming that in ozone the active atoms have united with the ordinary oxygen-molecule to form new complex molecules, the stability of ozone when kept at lower temperature, and especially the stability of the electro-negative condition of the active atoms, is more easily explained than on the assumption that the active atoms remain isolated. For as in general in chemical compounds each atom has a certain electrical condition, which is not arbitrary and variable, but belongs to the characteristics of the compound, so this may be presupposed of the oxygen-atoms united to a molecule; and it may be assumed in the present case that the active atoms are contained as electro-negative in the molecules, and must retain this electrical condition as long as they are in the compound.

It is not necessary in this case that the ozonized oxygen shall exhibit an electroscopically perceptible negative electrical tension. It may be assumed, as is done in the case of other chemically composed molecules, that the electrical conditions of the individual atoms of a molecule stand in such a relation to one another that the entire molecule is unelectrical; inasmuch as the algebraic sum of the quantities of free positive and negative electricity which the individual atoms of a molecule possess are exactly zero.

I must now speak of *antozone*.

In his investigations on oxygen, Meissner has made observations from which he concludes that, in the excitation of oxygen by electrical induction, along with ozone another substance is formed, which exerts a very remarkable action on aqueous vapour, and thereby betrays its existence. By further developing the subject, he believes he has convinced himself that this substance is nothing more than the modification of oxygen described by Schönbein as *antozone*.

The result, that in the isolation of oxygen by electrical induction *antozone* is formed simultaneously with ozone, agrees very well with my assumption that each molecule of ordinary oxygen consists of two opposite electrical atoms. But with reference to the somewhat different manner in which Meissner conceives the matter, namely that the atoms assume the opposite electrical condition first through electrical induction only, I would insist that the electrical antithesis of a molecule exists from the first, though it may possibly be heightened by induction. In reference to the separation of the two atoms, I quite agree with Meissner in thinking that it is most easily explained on the supposition that an electrical body exerts upon the two atoms forces which are opposed in direction.

Meissner has also made observations on the other modes of excitation of oxygen, which correspond to the previously mentioned one, and also favour the idea of the formation of ant-



ozone. The question is, What is the deportment of antozone, as far as it occurs in pure oxygen?

According to Meissner's observations, antozone, even in dry and cold oxygen, is less stable than ozone, inasmuch as, unlike this, it does not exist for an indefinite period, but gradually disappears—that is, is changed into ordinary oxygen. Hence it may be concluded that electro-positive oxygen-atoms, if they ever unite with the molecules of ordinary oxygen to form complex molecules, are less firmly retained in this compound than the electro-negative. To say more about the molecular constitution of antozone than that which follows from the above conditions, which must be fulfilled if the atoms in question shall occur as active oxygen, appears to be too bold in our present imperfect knowledge of the physical properties of ozonized oxygen.

And also regarding the question whether the atoms of antozone in pure oxygen retain their electro-positive condition as unchanged as in chemical combinations with other substances—for example, in peroxide of hydrogen and peroxide of barium—or whether and under what circumstances they lose the electro-positive, and approach the unelectrical condition, cannot, I think, be decided with certainty from the facts at present known. This question will have to be treated in connexion with that previously mentioned—that is, whether the atoms of antozone, like those of ozone, unite with ordinary oxygen to form complex molecules.

If ozone and antozone are simultaneously present in pure oxygen, a special molecular constitution may result, which is different from those which occur when only ozone, or only antozone occur. For it is conceivable that when an originally bi-atomic molecule has united with an electro-negative atom, it may thereby tend more to unite with an electro-positive atom, and that tetratomic molecules are thereby formed in which only two atoms form a pair. Both the other atoms may be in such positions that they do not come into direct contact with one another, and hence have no opportunity to unite and form a pair. In this case the latter atoms would satisfy the conditions above stated for active oxygen, that each can be separated as an individual atom from the molecule, and with a force which is smaller than that necessary to separate the atoms of a pair from one another. By such an arrangement of the atoms, the observation made by Meissner may perhaps be explained, namely, that antozone is more stable in dry oxygen if ozone is present than if it is not—a fact which at first sight seems to contradict the view that ozone and antozone tend to unite and form ordinary oxygen.

XLVII. *On the Application of the Vapour-density system of Notation to express Chemical Reactions.* By J. J. WATERSTON, Esq.

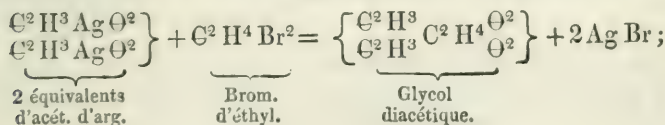
*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

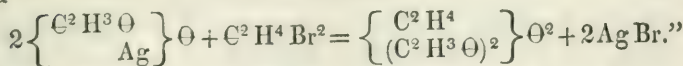
IN the following communication I give an example of the application of the vapour-density system of notation to express chemical reactions; and to place it in comparison along side of Gerhardt's system, I have selected the formulæ from M. Wurtz's memoir on the glycols (*Ann. de Chim.* vol. lv.), where that system is applied with great distinctness, and accompanied with vapour-density measurements. It appears from a passage in M. Wurtz's lecture on the Oxide of Ethylene, delivered before the London Chemical Society, June 5, 1862, that he considers Gerhardt's system as only provisional. \* \* \* "car les barres qui ne sont point superflues dans *ce moment de confusion*, ne sont que provisoires et devront disparaître par la suite" (*Ann. de Chim.* Nov. 1863, p. 361).

I have appended some remarks on the so-called hydrates of metals of the new school of chemists, as one of M. Wurtz's experiments has the appearance of favouring their views, but is, in my opinion, rather an example of the liability of being led astray by defective symbolism—defective because founded on substitutional rather than on physical characteristics.

P. 402 (*Ann. de Chim.* vol. lv.). "Supposons qu'on ait fait réagir sur 1 équivalent de bromure d'éthylène  $\text{C}^2\text{H}^4\text{Br}^2$ , par exemple, 2 équivalents d'acétate d'argent, il se formera par double décomposition du bromure d'argent et du glycol diacétique. Dans cette réaction, ces 2 équivalents de brome se sont portés sur les 2 équivalents d'argent, et le radical  $\text{C}^2\text{H}^4$ , se substituant à ces 2 équivalents d'argent dans les 2 molécules d'acétate d'argent, relie l'un à l'autre les restes de ces molécules de manière à former la nouvelle combinaison. On a, en effet,



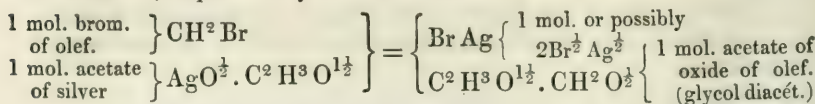
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*Phil. Mag.* S. 4. Vol. 27. No. 182. April 1864.

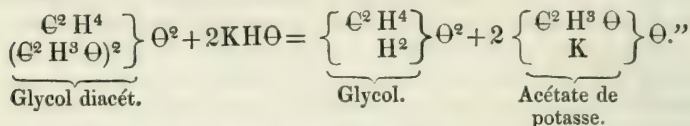
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The above, expressed by the V.D. notation, is

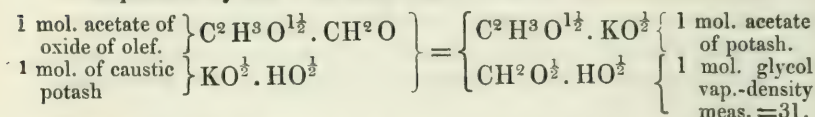


It will be remarked that the reaction consists of  $\text{CH}^2$  and Ag changing places.

P. 407. "*Décomposition du Glycol diacétique par l'hydrate de potasse sec.*"



Expressed by the V.D. notation, the above is

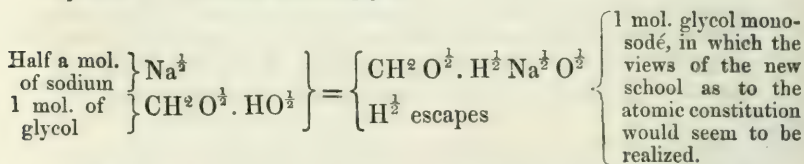


Heat is evolved while  $\text{KO}^{\frac{1}{2}}$  and  $\text{CH}^2 \text{O}^{\frac{1}{2}}$  change places.

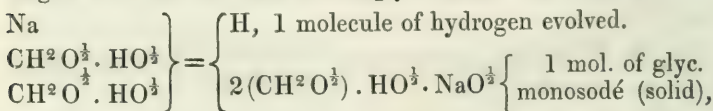
The symbol  $2\text{KH}\Theta$  employed by M. Wurtz to denote caustic potash, shows that he does not follow the new school in their views on the constitution of this body.

P. 412. "*Action du Potassium et du Sodium sur le Glycol.*"

11.5 parts by weight of sodium saturate 31 parts of glycol, during which heat is evolved, pure hydrogen is disengaged, and a white crystalline matter is formed, which is  $\text{C}^2 \text{H}^5 \text{NaO}^2$  "glycol monosodé," "dérivé du glycol par la substitution d'un équivalent de sodium à un équivalent d'hydrogen." This, expressed by the V.D., would seemingly be



But there is another arrangement possible, viz. 23 sodium to 62 glycol, or 1 molecule of sodium to 2 molecules of glycol uniting to form *one* molecule of the glycol monosodé. Thus

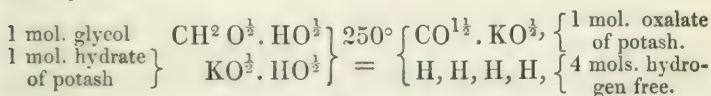


or dioxethylate of soda, consisting of 1 molecule of dioxethylene united to 1 molecule of the monohydrate of soda. The duplication of the oxide of ethylene molecule in such reaction will be admitted as extremely probable, although it cannot be tested by vapour-density measurement. See *Ann. de Chim.* vol. lxi. pp. 324, 326.

The glycol monosodé being melted with sodium at  $190^{\circ}\text{C}$ ., more hydrogen is evolved, and the white dry deliquescent glycol disodé,  $\left\{ \begin{smallmatrix} \text{C}^2\text{H}^4 \\ \text{Na}^2 \end{smallmatrix} \right\} \Theta^2$ , is obtained. This, by the V.D., is either  $2(\text{CH}^2\text{O}^{\frac{1}{2}}) \cdot 2(\text{NaO}^{\frac{1}{2}})$  one molecule, or  $2(\text{CH}^2\text{O}^{\frac{1}{2}} \cdot \text{NaO}^{\frac{1}{2}})$  two molecules.

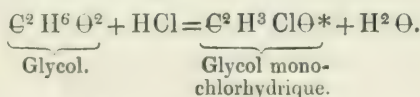
P. 417. The analysis of glycolate of silver is  $\text{C}=24$ ,  $\text{H}=3$ ,  $\text{Ag}=108$ ,  $\text{O}=48$ ; and since  $\text{Ag}=108 + \text{O}=8$  is  $\text{AgO}^{\frac{1}{2}}$  one molecule oxide of silver, we have  $\text{C}^2\text{H}^3\text{O}^{2\frac{1}{2}}$  as the representative of one molecule of glycolic acid, and the same represents the constitution of the acid in the analysis of glycolate of lime,  $\text{C}^2\text{H}^3\text{O}^{2\frac{1}{2}} \cdot \text{CaO}^{\frac{1}{2}}$ . The hydrated acid molecule is  $\text{C}^2\text{H}^3\text{O}^{2\frac{1}{2}} \cdot \text{HO}^{\frac{1}{2}}$ , and should not be represented by  $\text{CH}^2\text{O}^{1\frac{1}{2}}$ , which involves the absurdity of half a water-molecule.

P. 419. *Action of hydrate of potash on glycol* is represented "par l'équation suivante,  $\text{C}^2\text{H}^6\Theta^2 + 2\text{KH}\Theta = \text{C}^2\text{K}^2\Theta^4 + \text{H}^8$ ;" and by V.D.



A curious feature of this reaction is the decomposition of the water-molecules.

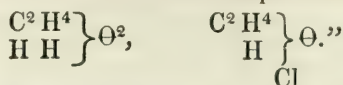
P. 419. *Action of muriatic acid on glycol*.—"Il a donné à l'analyse des résultats qui conduisent à la formule  $\text{C}^2\text{H}^5\text{Cl}\Theta$ . Ce produit résulte évidemment de l'action d'un seul équivalent d'acide chlorhydrique sur l'équivalent de glycol, et se forme en vertu de la réaction suivante



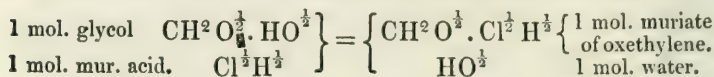
"J'ajoute que le glycol monochlorhydrique représente du glycol dans lequel 1 équivalent de chlore est venu prendre la place du



groupe HO. Les formules suivantes expriment ces relations :

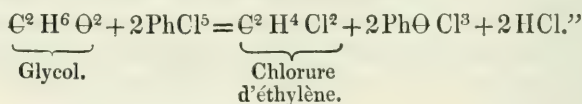


Expressed by the V.D. notation, the above reaction is

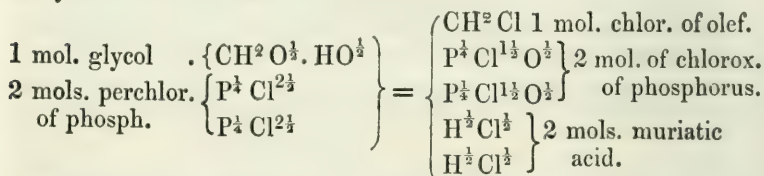


The reaction appears to be simply  $\text{Cl}^{\frac{1}{2}} \text{H}^{\frac{1}{2}}$ , driving out  $\text{HO}^{\frac{1}{2}}$  and taking its place: both molecules have their elements in a burnt condition. To say that  $\text{Cl}^{\frac{1}{2}}$  takes the place of  $\text{H}^{\frac{1}{2}} \text{O}^{\frac{1}{2}}$ , is to convey the idea of  $\text{Cl}^{\frac{1}{2}}$  leaving the acid-molecule  $\text{Cl}^{\frac{1}{2}} \text{H}^{\frac{1}{2}}$ , of which there is no evidence. It is a mode of expression that I do not think could be maintained if the potentialities of the phenomena were kept in view; yet it now pervades the writings of chemists.

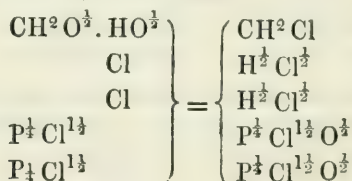
Pp. 419, 421. *Action of perchloride of Phosphorus on Glycol.*—Muriatic acid vapour is given off, and there remains the chlor-oxide of phosphorus and the Dutch liquid. “Ce chlorure prend naissance en vertu de la réaction suivante



By the V.D. notation the reaction is



The vapour-density of the perchloride of phosphorus, as measured by Mitscherlich, shows that although it boils at  $82^\circ \text{C}$ ., it is decomposed on rising into vapour, the one molecule,  $\text{P}^{\frac{1}{2}} \text{Cl}^{2\frac{1}{2}}$ , separating into two, viz.  $\text{P}^{\frac{1}{2}} \text{Cl}^{1\frac{1}{2}}$  and  $\text{Cl}$ ; so the above reaction may admit of being represented as



We have here the water-molecule of the glycol decomposed, its H separated into  $H^{\frac{1}{2}}$ ,  $H^{\frac{1}{2}}$ ; also one Cl separated into  $Cl^{\frac{1}{2}}$ ,  $Cl^{\frac{1}{2}}$ ,—these semimolecules ascending from the burnt condition and descending again to it; or they change their partners without changing their potential condition.

Pp. 430, 431. Ethyl-glycol, vapour-density measured  $45 = C^2 H^4 O^{\frac{1}{2}} \cdot HO^{\frac{1}{2}}$ .  $C^2 H^5 (C^2 H^5) O^2$  is the formula for this given at page 431.

P. 454. Butyl-glycol is isomeric with ethyl-glycol, and has the same vapour-density; its formula (page 455) is  $\left. \begin{matrix} C^4 H^8 \\ H^2 \end{matrix} \right\} O^2$ .

Diethyl-glycol, vapour-density measured  $59 = C^3 H^6 O^{\frac{1}{2}} \cdot HO^{\frac{1}{2}}$ .  $C^2 H^4 (C^2 H^5)^2 O^2$  is the formula for this given at page 432.

Glycol diacétique, vapour-density measured

$$73 = CH^2 O^{\frac{1}{2}} \cdot C^2 H^3 O^{\frac{1}{2}},$$

or one molecule anhydrous acetic acid united to one molecule oxide of ethylene. The formula for this given at p. 433 is

$$\left\{ \begin{matrix} C^2 H^4 \\ (C^2 H^3 O)^2 \end{matrix} \right\} O^2,$$

and for the same at p. 403 is  $\left\{ \begin{matrix} C^2 H^3 \\ C^2 H^3 C^2 H^4 O^2 \end{matrix} \right\}$ .

### Appendix.

In some educational books on chemistry lately published, *hydrates* of metals are freely spoken of; not potassium and sodium only, but of other metals as well: and the name is not merely typical, or given for classification purposes, but the constitution of the hydrated oxide is by the new school viewed (and the view generally inoculated into undergraduates?) as a molecule on which a single atom of oxygen, of hydrogen, and of the metal are united. Thus caustic potash is represented to be  $H^{\frac{1}{2}} O^{\frac{1}{2}} K^{\frac{1}{2}}$ , being isomeric with  $HO^{\frac{1}{2}} \cdot KO^{\frac{1}{2}}$ , which represents what is now called the view of the old school.

Mr. J. J. Griffin, in his 'Chemical Recreations,' 1834, appears to have first suggested this view, and he boldly announced his opinion that "*the hydrates of the metallic protoxides contain neither protoxide nor water.*" If the molecule of caustic potash is really  $H^{\frac{1}{2}} O^{\frac{1}{2}} K^{\frac{1}{2}}$ , it is evident that in it there is neither  $HO^{\frac{1}{2}}$ , an ultimate molecule or atom of water, nor  $KO^{\frac{1}{2}}$ , an ultimate molecule and atom of the protoxide. Mr. Griffin considered it absurd to suppose that water can exist in a body like caustic

potash, which stands red heat without change or giving off one particle of steam; but it is not likely that this argument has now much weight with chemists who agree with him otherwise because the intense heat given out during the union of the water with the oxide shows that a high degree of chemical action has taken place between them, and it cannot be expected under such circumstances that the physical characteristics of the water-molecule should be retained. The only way I can imagine the idea  $H^{\frac{1}{2}} O^{\frac{1}{2}} K^{\frac{1}{2}}$  to have taken root is from incompleteness in the conception of a compound molecule as an absolute entity. There is at least one distinct case where a compound molecule splits up like a simple molecule,  $N^{\frac{1}{2}} C^{\frac{1}{2}}$  appearing in compounds, but unknown in a separate form. There is also the organic proteus  $CH^2$ , which not only divides into two,  $C^{\frac{1}{2}} H$ , which does not exist in a separate form, but also appears in an indefinite number of multiple forms. There may be others, too; but no one upholds the possibility of  $HO^{\frac{1}{2}}$  being divisible into two atoms,  $H^{\frac{1}{2}} O^{\frac{1}{4}}$ , or  $KO^{\frac{1}{2}}$  into two,  $K^{\frac{1}{2}} O^{\frac{1}{4}}$ ; for although  $H^{\frac{1}{2}}$ ,  $K^{\frac{1}{2}}$  undoubtedly exist,  $O^{\frac{1}{4}}$  is not admitted even by the new school. The idea of substitution-equivalent which suggested and now upholds  $H^{\frac{1}{2}} O^{\frac{1}{2}} K^{\frac{1}{2}}$ , implies  $O^{\frac{1}{2}}$  as the ultimate atom of oxygen, and  $K^{\frac{1}{2}}$  as the substitute for  $H^{\frac{1}{2}}$  in  $HO^{\frac{1}{2}}$ . The vapour-density of caustic potash cannot be measured; but in coming to a decision as to whether the molecule of caustic potash is  $H^{\frac{1}{2}} O^{\frac{1}{2}} K^{\frac{1}{2}}$  or  $H O K$ , it is well to keep in view that the K in  $KO^{\frac{1}{2}}$  and the H in  $HO^{\frac{1}{2}}$  must be decomposed in the former case, and not in the latter. There is also to be considered the intensity of the force that unites the molecules of the oxide together: their cohesion-integral must be very great, since the heat of a wind-furnace liquefies it, but does not volatilize it. We have an idea of the dynamic value of the force that unites the K with  $O^{\frac{1}{2}}$ , from the heat of combustion evolved in burning K. In the production of water from the union of H with  $O^{\frac{1}{2}}$  to form  $HO^{\frac{1}{2}}$  steam, and the union of  $HO^{\frac{1}{2}}$  with  $HO^{\frac{1}{2}}$ ,  $HO^{\frac{1}{2}}$ ,  $HO^{\frac{1}{2}}$ , &c. to form water  $HO^{\frac{1}{2}}.HO^{\frac{1}{2}}.HO^{\frac{1}{2}}.$  &c., the dynamic value of each of these two forces has been measured, and the ratio of the chemical to the cohesive integral found to be about 7 to 1. There are cohesive affinities of vast potency as well as chemical; and we have to consider whether it is more probable that, when water and the oxide are brought into contact, the heat evolved (which indicates molecular force expended) is due to cohesive affinity between  $KO^{\frac{1}{2}}$  and  $HO^{\frac{1}{2}}$ , or to chemical

reactive force,  $\left\{ \begin{smallmatrix} K^{\frac{1}{2}} O^{\frac{1}{2}} K^{\frac{1}{2}} \\ H^{\frac{1}{2}} O^{\frac{1}{2}} H^{\frac{1}{2}} \end{smallmatrix} \right\} = \left\{ \begin{smallmatrix} H^{\frac{1}{2}} O^{\frac{1}{2}} K^{\frac{1}{2}} \\ K^{\frac{1}{2}} O^{\frac{1}{2}} H^{\frac{1}{2}} \end{smallmatrix} \right\}$ , which effects a crossing

over of half molecules between the water-molecule and the oxide-molecule. If we decide on cohesive affinity, we may retain the name hydrate of potassa. If we decide on chemical reaction, we must find a different name; but hydrate of potassium will not do, because the water has been decomposed and no longer exists in the half-molecule compound. Potassic water does not seem an improvement, though it manifests the desire of the new school to hold on to the name while rejecting the thing named.

The atomic theory derived its origin from the quantitative relation of the substitution equivalents in chemical reactions. The idea of equivalent had then to give way to that of fixed atomic weight, or smallest combining ratio. Next, these were found to have a simple relation to vapour-density, also to specific heat. The last step has been the dynamic theory of heat and gases, which represents the law of volumes as the strictly physical sequence of an equilibrium of pressure and *vis viva*, or temperature, between perfectly elastic particles or molecules of different weights, and the law of specific heat in simple solid bodies as the probable sequence of the independent motion of the chemical atoms while maintaining an equilibrium of *vis viva*. By measuring the specific heat of elementary solids, we are enabled to obtain approximately the smallest combining proportion or atom of these solids in terms of a constant unit. By measuring the specific gravity of a gas or vapour, we obtain the weight of the molecule or smallest part that exists in a free or separate form in terms of a constant unit. We thus come to know that the molecule in a certain class of bodies contains only one atom, in another class two atoms, in a third class four atoms, and that the molecule of one body, sulphur, contains six atoms; but at a very high temperature this large molecule is separated into three smaller molecules, each of which contains two atoms. Mercury and cadmium are known, from measurement of vapour-density, to have one-atomed molecules; and it is inferred that most of the metals, such as iron, zinc, copper, lead, &c., have also one-atomed molecules, because they also combine with oxygen in the ratio of one specific-heat atom to an atom of oxygen. But there are four, viz. silver, lithium, sodium, and potassium, which combine with oxygen in the ratio of two specific-heat atoms of metal to one of oxygen, so that their vapour-densities might perhaps show them to have two-atomed molecules. The dual or two-atomed nature of the molecules of the elements oxygen, hydrogen, &c. is of high interest in regard to the dynamic integral of the force that unites the  $O^{\frac{1}{2}}$  with  $O^{\frac{1}{2}}$ , the  $H^{\frac{1}{2}}$  with  $H^{\frac{1}{2}}$ : *e. g.* we may





the transference is made from the embrace of the oxygen to the embrace of the metal without any appearance of the full equivalent of force that would be generated if the metallic molecule had descended from the vaporous or separate-molecule form.

An atom of metal seems almost as indisposed to exist in a state of isolation as an atom or half-molecule of gaseous element.

To attempt to express such conditions by modifying the notation, would probably only lead to confusion. But I submit that the burnt or unburnt, the clasped or raised condition of atoms, should be kept well in view while studying chemical reactions; otherwise we shall be apt to lose sight of their real potential nature and wander into a maze of false analogy, barren of any result but an unmeaning jargon of words. For the same reason I submit that chemical notation should be founded on physical relations only, now that the classical labours of modern physicists have supplied nearly all that is necessary to do so.

J. J. WATERSTON.

Edinburgh, February 21, 1864.

XLVIII. *On Fractional Values for the Heptagon and Circle.*

By HENRY NORTON, Esq.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

YOUR Magazine for November 1863, at page 408, contains a note by Mr. Drach on an approximate construction of the heptagon; and the Number for last February, at page 124, contains a paper by Sir W. R. Hamilton "On Röber's Construction:" I wish through you to point out to those gentlemen a third construction of remarkable simplicity and accuracy,

$$\frac{s}{r} \cdot \frac{\pi}{7} = \frac{105}{242} = \frac{3 \times 5 \times 7}{11 \times 11 \times 2}, \text{ or for construction } = \frac{\frac{3 \times 5}{2}}{\frac{11 \times 11}{7}}.$$

This value of  $\sin \frac{\pi}{7}$  is . . . . 43388,42975 . . .

The true value, according to Sir }  
W. Hamilton, is . . . . }

43388,37391

Error . . . . 00000,05584 . . .

Error of H. Röber's  $s \cdot \frac{\pi}{7}$  . . . 00000,01579 . . .

In strictness the latter is the more correct; but practically, taking into account the much greater simplicity of the construc-

tion, I doubt whether the former would not be found fully as accurate as the latter.

I have constructed Röber's figure from the quantities given by Sir William, and also

$$8x^3 + 4x^2 - 4x - 1 = 0, \text{ and } z^3 + z^2 - 2z - 1 = 0$$

in the forms

$$\frac{x\left(x + \frac{r}{2}\right)}{\frac{r}{2}} = \frac{r\left(x + \frac{r}{4}\right)}{x} \text{ and } \frac{\frac{z}{2}(z+r)}{r} = \frac{r\left(z + \frac{r}{2}\right)}{z},$$

the latter by placing the chord of  $\frac{4\pi}{7}$  in two intersecting circles.

I think I can understand the whole train of thought which led to Röber's plan, and I certainly find no evidence of any attempt to do more than to discover a coincidence. I have not seen Röber's book.

Supposing that it were proved that the heptagon did enter into the plans of ancient architects, it must have been attributable to some superstitious reverence for the figure itself: reverence for the number 7 would in all probability lead to a thorough investigation of the polygon, not to mention that it was the first polygonal figure which resisted all their attempts to find a formal method of construction. How were they likely to have attempted such an investigation in such early times? The earliest astronomy consisted in a long and patient observation of the periods of the heavenly bodies, and in a careful examination of the ratios of those periods one to another in order to obtain some simple fraction. This would appear the more mysterious in proportion as the numbers were low: continued fractions probably owed their origin to such inquiries. They would also reduce those fractions to their prime factors, in order to see at a glance whether they were capable of simple relations with other quantities or not.

If this was their practice with regard to time, nothing is more likely than that they would apply the same method to space: any two lines whose ratio was incapable of rigorous demonstration would be subjected to actual measurement. A very large figure, constructed either tentatively or by approximation, would give them the measures with considerable accuracy; and having them, they had a chance (by no means despicable) of finding the above fraction, although not equal to my own chance, with Hutton's Tables and Goodwyn's 'First Centenary of Decimal Quotients'\*. I think they are more likely to have done this than

\* The actual fraction is not there; the Table goes only from  $\frac{1}{1000}$  to  $\frac{1}{10}$ .

to have set to work with rule and compasses to find coincidences. They certainly used rule and compass in legitimate analysis; only they seem to have required "a kind of mental contrivance and construction to form a connexion between the data and quæsitæ"\*, which is not visible in Röber's figure; at least his quæsitum is not demonstration. I think it is not unlikely that the earliest mathematicians may have become acquainted with the fraction, and not impossible that they may have constructed Röber's figure. Meanwhile it must not be forgotten that in all probability they could by approximation *construct any angle* they pleased, although they might be unable to calculate the chord. Bisection is quite elementary; and with this power and a table of the powers of two, nothing was easier than to construct a heptagon with any desired degree of accuracy, with full knowledge of the amount of error and with the power to correct it tentatively. Surely if they were mathematicians at all they were capable of a statement like this,

$$2^{12} = 4096 = (585 \times 7) + 1 \text{ and } \frac{1}{7} - \frac{585}{4096} = \frac{1}{4096} \text{ error,}$$

$$2^{15} = 32768 = (4681 \times 7) + 1 \text{ and } \frac{1}{7} - \frac{4681}{32768} = \frac{1}{32768} \text{ "}$$

$$2^{18} = 262144 = (37449 \times 7) + 1 \text{ and } \frac{1}{7} - \frac{37449}{262144} = \frac{1}{262144} \text{ "}$$

and so on to any required degree of accuracy. For construction,

$$\frac{1}{7} = \frac{1}{2^3} + \frac{1}{2^6} + \frac{1}{2^9} + \frac{1}{2^{12}} + \frac{1}{2^{15}} + \frac{1}{2^{18}} + \frac{1}{7 \cdot 2^{18}} \text{ of the circle.}$$

Or the construction might have been commenced with the side of a hexagon or decagon.

On this subject I would have consulted Pappus if the work had been accessible to me: I only know from the article in Hutton's Dictionary, that in his 4th Book he shows how to trisect an angle, and to divide a given arc or angle in any given ratio. By some such method early mathematicians could have described a heptagon; but something else was necessary to clothe it with superstitious respect.

I wish to submit to your consideration an hypothesis connected with the Indian value of the circle  $\frac{3927}{1250} = 3.1416$ . I suspect that when they found themselves baffled in attempting to calculate the chords on successive subdivisions, they resorted to a series of rational right-angled triangles ( $2x$ ,  $x^2 - 1$ ,  $x^2 + 1$ ), constructed large figures, and hoped to find

\* Hutton's Mathematical Dictionary, under "Analysis."



some such side exactly contained either in one circle or in several circles; and when they thought they had made the discovery, if they wished for further accuracy they commenced their bisection with it. It is a curious fact that the most simple fractional value of the circle  $\frac{355}{113}$  is founded on a diameter which forms the hypotenuse of one of these triangles (15, 112, 113). It represents an angle of about  $7^{\circ}657875$ , of which  $47\cdot195\dots$  constitute a circle. It was therefore of no use for the experimental determination of the value of the circumference. Probably they did not attempt to measure the circumference itself, not being able to do it with sufficient accuracy, but confined themselves to measurement by inscribed polygons; this would account for their not discovering the above fraction if really they were not acquainted with it. But there was another rational right-angled triangle, 100, 2499, 2501, with an angle of about  $2^{\circ}291526$ . The diameter being 2501, the chord would be 100, of which 1571 would very nearly compose 20 circles, the angle being contained about  $157\cdot10054\dots$  times in a circle. Now, 157 being greater than ( $2^7=128$ ), this side, compared with the side of an inscribed square, would save them seven bisections; as compared with that of a circumscribed square, 6 bisections; and about as many compared with those required in commencing with the hexagon. Designating the angle of the above triangle by A, its half by B, and its quarter by C, I find

$$\cos B = \sqrt{r \frac{r + \cos A}{2}} = 2500\cdot499\ 950\ 199\ 570,$$

$$\sin C = \sqrt{r \frac{r - \cos B}{2}} = 25\cdot006\ 244\ 728\ 82,$$

$$\sin C \times 4 \times \frac{157\cdot1}{2} = 7856\cdot962\ 093\ 795\dots$$

Seeing how nearly this approached to 7857, and knowing that the actual value must be somewhat greater, I suspect they determined their first value at  $\frac{7857}{2501}$ ; then finding the odd unit in the denominator inconvenient, they altered the numerator by 3, in the rough proportion of  $\frac{3}{1}$ , making  $\frac{7854}{2500} = \frac{3927}{1250}$ . This is  $=3\cdot1416$ , a much better fraction than the one they had calculated ( $=3\cdot14152987\dots$ ), or the approximate value

$$\frac{7857}{2501} = 3\cdot141543\dots$$

It is very likely that they made a third bisection, which would justify and may even have suggested the change into  $\frac{3927}{1250}$ .

Certainly if they could have calculated nine bisections of the side of a circumscribed square, they would have actually obtained  $3.1416025 \dots$ ; or from nine divisions of the side of a hexagon they would have obtained a value slightly in excess of  $3.14159$ , as nearly as I can determine from  $s \left( \frac{\pi}{6 \times 2^9} = 3' 30'' \frac{15}{16} \right)$ , with only seven places of logarithms. But in this latter case at least, if they worked with a decimal diameter of  $1.000 \dots$ , why should they put  $3.1416$  for  $3.14159$ ? Why use a vulgar fraction at all? There are three probabilities thus suggested in favour of the rational right-angled triangle. (1) The fractional form. (2) The difficulty of calculating so many bisections. (3) The probability that they would try to measure the circle experimentally by a many-sided polygon; and I might add that they probably hoped, even if the circumference and radius were incommensurable, to find some rational right-angled triangle one side of which would exactly measure either one circle or a small number of circles.

Unless some historical hint of the fraction  $\frac{7857}{2501}$  could be found, this must remain a mere conjecture, and pass for what it is worth.

I am, Gentlemen,

Your obedient Servant,

Norwich, March 16, 1864.

HENRY NORTON.

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XLIX. *On the Influence of the Tidal Wave on the Earth's Rotation, and on the Acceleration of the Moon's Mean Motion.* By Mr. JAMES CROLL\*.

IT has been shown by M. Mayer that the tidal wave tends to diminish the earth's rotation. The rotation of the earth carries the wave a little to the east of the meridian, over which the moon is raising it. And as the attraction of the moon tends continually to pull the wave back to the meridian against the direction of rotation, the wave must act as a drag upon the earth's motion. As the drag acts by friction, the *vis viva* of rotation is converted into heat.

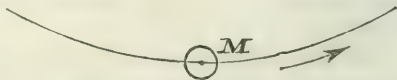
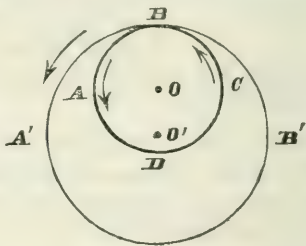
There is, however, another way in which the tidal wave tends to diminish the earth's rotation, which does not appear to have been noticed by physicists. But in order fully to comprehend

\* Communicated by the Author.

the subject, it will be necessary, for the following reasons, to enter somewhat minutely into one or two points regarding the cause of the tides.

The existence of the tidal wave depends upon the action of two forces, viz. centripetal and centrifugal force. In the production of the lunar wave, for instance, we have to consider, first, the influence of the attraction of the moon on the water of the ocean; and secondly, the influence of the centrifugal force of the earth's motion round the common centre of gravity of the earth and moon. Or rather, we should say, we have to consider the combined effect of these two forces on the waters of the ocean. Although in every mode of explaining the production of the tidal wave the element of centrifugal force must be recognized, yet it is not generally stated under that particular form best adapted to exhibit the influence of the wave on the *vis viva* of the earth's rotation. That clear conceptions on the relation of centrifugal force to the tidal wave are necessary, is evident if we but reflect that the very subject of our present inquiry is the retarding influence exerted by the wave on the centrifugal force of rotation.

The earth's mass is to that of the moon's as 87·7 to 1. The mean distance between their centres being 237,000 miles, the centre of gravity around which they both revolve is situated 2702 miles from the earth's centre, or 1261 miles below its surface on the side next the moon. Let ABCD represent the earth's equator, and O its centre or axis on which it rotates; M the moon, and O' the centre of gravity of the earth and moon, around which they both revolve. A'B' will represent the path which the earth's surface at the point B opposite to the point B opposite to the moon will take as it moves round the common centre of gravity O'. It is found that the centrifugal force occasioned by the earth's rotation on its centre O, causes the waters of the ocean to rise at the equator ABCD to the height of 34,950 feet; this being the difference between the equatorial and the mean radius of the earth. Now if the rotation of the



earth on the centre O causes the water of the ocean to rise 34,950 feet at the point B, then a rotation on the centre O' must also cause the water to rise at the same point. The same mechanical cause which produces the effect in the first case must operate in the second. This is perfectly evident. As O', the common centre of gravity of the earth and moon, is situated in the interior of the earth, all parts of the earth must move round this point once a month as truly as they do round the centre O once a day. The point B being further from the centre of rotation than any other point on the surface of the globe, the waters of the ocean will rise higher at this place than anywhere else. This rise constitutes what is called the tidal wave. The wave of course remains permanently situated at the point B, for this point never changes either in relation to the moon or to the centre of rotation. But as the earth revolves round the centre O once a day, all parts of the equator must within that period pass under this point. The earth's surface at the equator must be moving under the tidal wave at a rate of 1526 feet per second, and consequently the waters of the ocean must be continually rising so as to maintain the wave continuously at B. At the point D, on the side toward the moon, the centrifugal force will be but small, this point being situated only 1261 miles from the centre of rotation. But it will be observed that the attraction of the moon at this place is not only stronger than at B, but is also acting in the same direction as the centrifugal force. Consequently the waters must rise at D also. At the points A and C the centrifugal force acts in lines parallel to the earth's surface; hence the waters at these places cannot rise, they will simply recede in the direction of the point B.

That the tidal wave must rise to the same height on both sides of the earth will appear evident from the following considerations.

Let us suppose that our globe consists entirely of a fluid mass revolving with a uniform velocity round the common centre of gravity of the earth and moon once a month. We shall in the mean time leave out of consideration the diurnal motion of rotation. It can be easily demonstrated that such a fluid mass would, under the specified conditions, assume the form of an oblong spheroid with its greater axis pointing in the direction of the moon. In other words, the waters on the side furthest from the moon would, under the influence of centrifugal force, recede to a certain extent; and the waters on the opposite side would, under the influence of centripetal force, be drawn toward the moon to the same extent. Here we have the rise constituting the tidal wave on both sides of the earth. Suppose now that the earth, instead of being a fluid mass, consists, as it really



does, of a solid body or nucleus surrounded by a fluid mass. It is evident that the fluid mass surrounding the solid earth would assume identically the same shape as it would do supposing there were no solid matter. Now in order to know whether the tidal wave rises to the same height on both sides of the globe or not, we have merely to determine whether or not the centre of gravity of the surrounding fluid mass coincides with the centre of gravity of the enclosed solid mass. That the two centres must coincide is evident from the fact that the centre of gravity of any mass moving uniformly round a distant centre must remain always at the same distance from that centre, whatever form the mass may assume. The distance of the centre of gravity of the mass from the centre of rotation is determined by the velocity of the motion of the former centre round the latter. Now the centre of gravity of the surrounding fluid, and the centre of gravity of the solid mass, are in the present case moving with the same velocity. Hence both centres must be at the same distance from the centre of rotation, and consequently the solid mass must be exactly in the centre of the surrounding fluid mass. It follows that the depth of the waters, or, which is the same thing, the height of the tidal wave on the opposite sides must be the same.

The following is a very simple mode of calculating the height to which the tidal wave will rise at the point B. The distance of the point B from the centre O' is 6665 miles, and it performs one revolution in 27 days 7 hours 43 minutes and 11 seconds. This point therefore moves round the common centre of gravity O', of the earth and moon, at the rate of 93·6 feet per second. The same point in its diurnal motion round the earth's axis O moves at the rate of 1526 feet per second, this being the velocity of the earth's diurnal rotation at the equator. The height to which the tidal wave will rise at the point B will of course be proportional to the centrifugal force at that point. Now the centrifugal force is as the square of the velocity. And as 34,950 feet is the height corresponding to a velocity of 1526 feet per second, 130·7 feet must be the height corresponding to a velocity of 93·6 feet per second; for

$$(1526)^2 : (93\cdot6)^2 :: 34950 : 130\cdot7.$$

But B is much further from the centre O' than from the centre O; and as the centrifugal force is in the inverse ratio of the distance from the centre of rotation, we have then

$$6665 : 3956 :: 130\cdot7 : 77 \text{ feet.}$$

This is the height to which the tidal wave would rise at the point B did the moon move in the plane of the earth's equator, and were the entire globe covered with water to the mean depth of the ocean, and none of the centrifugal force wasted in friction

when raising the water. In regard to the extent of the rise at the equator, occasioned by the earth's diurnal motion, it is of little consequence whether the earth be uniformly covered with water or not, for in this case the water remains permanently raised; but in regard to the rise occasioned by the earth's rotation on the centre  $O'$  the case is entirely different. Here the rise only exists at the two places  $B$  and  $D$  in the form of an exceedingly broad wave, fixed in relation to the position of the moon; and as the waters at these places are continually shifting at a rate of upwards of a thousand miles an hour, there must be a constant rise and fall so as to maintain the tidal wave in its proper place. But whatever breaks up the continuity of the ocean, such as an island or a continent, must stand as an obstacle in the way of the waters rising, and will thus prevent the wave attaining anything like the height corresponding to the centrifugal force.

That which makes the waters rise at  $B$  is, as we have seen, the centrifugal force occasioned by the earth's motion round this centre  $O'$ . But it is impossible that the waters of the ocean can rise without a considerable portion of the force applied being consumed in friction, in the rubbing of the molecules of the water against each other or against the sea-shore, or against any other obstacle standing in their way. And whatever is lost in friction goes to produce heat, and can never again be restored to its source. But what is its source? What is the motion which is thus consumed in friction? It is simply the motion of the molecules of the water round the centres  $O$  and  $O'$ ; for at this moment they are moving round both centres. It will be observed that the molecules of the water situated, say at the point  $C$ , are, in consequence of the earth's diurnal rotation, carried in the direction of  $B$ . But as they approach to  $B$ , they are receding from the other centre of rotation  $O'$ , and consequently have the velocity of their motion round this centre increased. This motion reaches a maximum when the molecules arrive at the point  $B$ . By this time the centrifugal force has become so much increased as to cause them to fly off in a tangent to the surface of the earth, to a certain extent, and thus the tidal wave is formed. It is perfectly evident that the solid parts composing the earth's surface would fly off along with the molecules of the water if they could. But this they cannot do, because they are rigidly fixed to the earth by the force of cohesion. The molecules of the water as they recede will therefore pass over the solid particles of the earth, and in doing so will rub against them, and thus lose a portion of their motion. But the motion lost, be it observed, is the motion of revolution round the centres  $O$  and  $O'$ . If the motion of the molecules of the water had been simply

transferred to the solid particles of the earth against which they rubbed, then the earth, as a whole, would not have lost any of its *vis viva* of rotation. But the motion of the molecules is transformed into heat-vibrations, and thus lost in so far as rotation is concerned.

It will be observed that at the time when the molecules of the water are losing their motion, they are moving round both the centre O and the centre O'. The question now suggests itself, whether is the heat produced at the expense of the motion round the centre O, or round the centre O'? If we assume that it is at the expense of the latter, that is to say, at the expense of the earth's motion round the common centre of gravity of the earth and moon, then we must conclude that the velocity of the earth's motion round this centre is diminishing; and if diminishing, then the earth must be falling towards this centre; or, in other words, falling towards the moon. And if the earth and moon are approaching toward each other, then the velocity of their motion round this centre must be increasing. But to assume that the friction of the waters in this case could have any influence on the general motion of the earth and moon round their common centre, seems to be in opposition to the principle that the general motion of a system cannot be affected by the relative motion of its parts. Hence we infer that the entire heat produced in the present case is derived from the motion round the centre O; in other words, from the diurnal rotation of the earth.

It will be observed that the way in which we have explained how the heat is produced, and consequently the earth's diurnal motion destroyed, is entirely different from that pointed out by Mayer. According to his view, when the wave passes the meridian or point B, the moon, as it were, takes hold of it and pulls it back; the solid part of the earth at the same time moving forward under the wave, rubs against its bottom and thus generates heat. This rubbing process acts as a drag upon the rotation of the solid earth and thus consumes its motion. In this case it is the solid part of the earth that loses motion by being held back by the molecules of the water. In the case to which we have directed attention, the reverse takes place. It is the molecules of the water that lose their motion by being held back by the solid part of the earth. But the general effect on the earth's rotation is the same in both cases. For, in so far as the globe is concerned, it is the same whether it be the sea or the dry land which in the first instance loses its motion.

M. Mayer is perfectly correct in his explanation; but then he only exhibits one part of the process. It will be observed that the heat is produced, according to his explanation, when the solid parts of the earth are endeavouring to move faster than



the molecules of the water. Now this effect cannot take place while the waters are rising; for it is the effort of the molecules of the water to pass on before the solid particles that causes these molecules to rise up in the form of a wave. But we have shown that they lose motion in this endeavour, and by losing it, the earth loses its motion of rotation. Again, according to Mayer the earth can lose its motion of rotation only when the molecules of the waters, forming the tidal wave, are descending; for it is only then that they can possibly be moving slower than the solid parts of the earth. But, according to the explanation just given, the consumption of rotation takes place also when the molecules are rising. And again, according to Mayer the effect cannot take place until the molecules of the water have passed the point B, the meridian. According to our explanation, the effect takes place before they reach the meridian, and also at the meridian, as well as beyond it. In short, the effect takes place, as we have seen, wherever the waters rise, no matter where this may happen to be. The one explanation, as we have already stated, is not antagonistic to the other. Both are correct. Mayer explains how the earth loses its motion of rotation while the waters are descending to their normal level. We, on the other hand, have explained how the same effect takes place while the waters are rising.

Although the lunar wave does not diminish the *vis viva* of the earth's motion round the centre  $O'$ , we shall find, however, that the same is not the case in regard to the influence of the solar wave. The tidal wave produced by the influence of the sun affects, although to a less extent, the earth's rotation in the same manner as the wave produced by the moon. But, in addition to this, it evidently exerts a retarding influence on the rotation of the earth and moon on their common centre of gravity  $O'$ , and consequently tends to accelerate the moon's mean motion. The following considerations will, we doubt not, render this obvious.

To simplify our subject, let us suppose the earth to be moving round the sun, unaccompanied by the moon. In this case the velocity of the earth's motion will correspond to the centripetal force of the sun's attraction at the distance of the earth's centre of gravity from that body. The waters on the side furthest from the sun, moving in an orbit nearly 4000 miles beyond that of the earth's centre, will require, in order to prevent them from receding from the earth, to move slower than that centre; but instead of this, they actually move swifter, being further removed from the centre of rotation. The consequence is, the waters of the ocean rise at this place, and constitute what we call the solar wave, on the side furthest from the sun. The waters would separate from the earth altogether, were it not that they are



bound to it by the force of gravity. They, however, rise at this point till the centrifugal force causing them to recede, becomes balanced by the earth's gravity pulling them back. The side turned towards the sun, however, is moving in a path nearly 4000 miles nearer to the sun than that of the earth's centre. Hence, in order to prevent the waters at this place from falling towards the sun, they must move with greater velocity than the centre. But instead of this, they move with less velocity, and the consequence is, the centrifugal force is not sufficient to balance the centripetal force of the sun's attraction. The waters, therefore, fall away from the earth toward the sun, and thus a solar tide is also produced on the side nearest the sun.

Let us suppose the moon now to accompany the earth in its orbit. Everything in this case still happens as we have described, with this difference, however, that it is not the earth's centre of gravity, but the common centre of gravity of the earth and moon which now moves in the true orbit. In order to convince ourselves that the heat produced by the solar wave is not altogether at the expense of the earth's diurnal rotation, but must be partly at least at the expense of the earth's rotation on the common centre of gravity of the earth and moon, we have merely to reflect on what would take place were the earth to be deprived of its diurnal motion of rotation. We should then have a tidal wave produced by the sun the same as before, but it would move round the earth only once in a month. But notwithstanding the slow motion of the wave, the waters would still have to rise and fall the same as formerly, in order to maintain the tidal wave continuously in its true position in relation to the sun, as the earth rotates on its centre  $O'$ . Now the force, which in this case would be converted into heat by the friction of the waters as they rise and fall, could not be derived from the earth's diurnal rotation, for in the present case no such motion exists. Hence we must infer that it is produced solely at the expense of the earth's motion round the common centre of gravity  $O'$  of the earth and moon.

Suppose that the earth should now begin to rotate on its own axis, it is perfectly evident that the rotation will exercise no influence in the way of preventing the existence of those effects which we have shown take place independently of the rotation. Other effects will, no doubt, follow from the earth's diurnal motion. But those previously occurring will still take place the same as formerly. But if heat is produced at the expense of the earth's motion round the centre of gravity of the earth and moon, then there must be a continual consumption of the *vis viva* of motion round this centre; for the heat produced can never be all restored to its source. And if the *vis viva* of the earth's mo-

tion round this centre be diminishing, then the earth must be falling toward it; or, in other words, approaching toward the moon. And if so, then the velocity of the earth's motion round this centre must be increasing. Hence we arrive at the conclusion that the tidal wave produced by the sun *tends to accelerate the moon's mean motion*\*.

This conclusion is apparent in another way. If we admit that the tidal wave, produced by the influence of the moon, affects the rotation of the earth on its axis  $O$ , then we have every reason to conclude that the tidal wave produced by the sun will affect the rotation of the earth on the common centre of gravity  $O'$  of the earth and moon. For the rotation of the earth and moon, on their common centre, stands in exactly the same relation to the sun as the diurnal rotation of the earth on its own centre  $O$  stands to the moon. For the earth and moon may be regarded as but one body revolving on its axis  $O'$  in presence of the sun.

It was shown by Laplace that the slow diminution which is taking place in the excentricity of the earth's orbit tends to accelerate the moon's motion. But this change, like all the other great changes in celestial motion, is periodic. For when the earth's orbit becomes almost a circle, and again begins to open out into an ellipse, a retarding influence on the moon's motion will have commenced. The reason why all such motions are periodic is this; there is no real loss of force: there is merely a transference of force from one part of the system to another. The elements of ultimate restoration consequently still remain. But when the *vis viva* of celestial motion becomes transformed into heat, things never can be brought back to their former position; for a portion of this heat is always radiated into space and lost for ever.

Now, if the *vis viva* of the earth's motion on its centres  $O$  and  $O'$  is being transformed into heat by the action of the tidal wave, the diurnal motion of our globe will, in the long run, not only be totally destroyed, but the earth and moon will ultimately fall together and revolve round the sun as one planet.

\* Since the above was written, I have been informed by Prof. William Thomson that he has arrived at results somewhat similar. He finds, however, that the influence of the tidal wave is to cause the moon in the first instance to recede from the earth. But the ultimate tendency is to bring the sun, moon, and earth together, and cause them to rotate as one rigid body on its axis. The conclusion arrived at by this able physicist will, however, be shortly laid before the public.

L. *On the Calculation of the Equilibrium and Stiffness of Frames.*  
 By J. CLERK MAXWELL, F.R.S., Professor of Natural Philosophy in King's College, London\*.

THE theory of the equilibrium and deflections of frameworks subjected to the action of forces is sometimes considered as more complicated than it really is, especially in cases in which the framework is not simply stiff, but is strengthened (or weakened as it may be) by additional connecting pieces.

I have therefore stated a general method of solving all such questions in the least complicated manner. The method is derived from the principle of Conservation of Energy, and is referred to in Lamé's *Leçons sur l'Elasticité*, Leçon 7<sup>me</sup>, as Clapeyron's Theorem; but I have not yet seen any detailed application of it.

If such questions were attempted, especially in cases of three dimensions, by the regular method of equations of forces, every point would have three equations to determine its equilibrium, so as to give  $3s$  equations between  $e$  unknown quantities, if  $s$  be the number of points and  $e$  the number of connexions. There are, however, six equations of equilibrium of the system which must be fulfilled necessarily by the forces, on account of the equality of action and reaction in each piece. Hence if

$$e = 3s - 6,$$

the effect of any external force will be definite in producing tensions or pressures in the different pieces; but if  $e > 3s - 6$ , these forces will be indeterminate. This indeterminateness is got rid of by the introduction of a system of  $e$  equations of elasticity connecting the force in each piece with the change in its length. In order, however, to know the changes of length, we require to assume  $3s$  displacements of the  $s$  points; 6 of these displacements, however, are equivalent to the motion of a rigid body so that we have  $3s - 6$  displacements of points,  $e$  extensions and  $e$  forces to determine from  $3s - 6$  equations of forces,  $e$  equations of extensions, and  $e$  equations of elasticity; so that the solution is always determinate.

The following method enables us to avoid unnecessary complexity by treating separately all pieces which are additional to those required for making the frame stiff, and by proving the identity in form between the equations of forces and those of extensions by means of the principle of work.

#### *On the Stiffness of Frames.*

*Geometrical definition of a Frame.*—A frame is a system of lines connecting a number of points.

\* Communicated by the Author.



A stiff frame is one in which the distance between any two points cannot be altered without altering the length of one or more of the connecting lines of the frame.

A frame of  $s$  points in space requires in general  $3s-6$  connecting lines to render it stiff. In those cases in which stiffness can be produced with a smaller number of lines, certain conditions must be fulfilled, rendering the case one of a maximum or minimum value of one or more of its lines. The stiffness of such frames is of an inferior order, as a small disturbing force may produce a displacement infinite in comparison with itself.

A frame of  $s$  points in a plane requires in general  $2s-3$  connecting lines to render it stiff.

A frame of  $s$  points in a line requires  $s-1$  connecting lines.

A frame may be either simply stiff, or it may be self-strained by the introduction of additional connecting lines having tensions or pressures along them.

In a frame which is simply stiff, the forces in each connecting line arising from the application of a force of pressure or tension between any two points of the frame may be calculated either by equations of forces, or by drawing diagrams of forces according to known methods.

In general, the lines of connexion in one part of the frame may be affected by the action of this force, while those in other parts of the frame may not be so affected.

### *Elasticity and Extensibility of a connecting piece.*

Let  $e$  be the extension produced in a piece by tension-unity acting in it, then  $e$  may be called its extensibility. Its elasticity, that is, the force required to produce extension-unity, will be  $\frac{1}{e}$ .

We shall suppose that the effect of pressure in producing compression of the piece is equal to that of tension in producing extension, and we shall use  $e$  indifferently for extensibility and compressibility.

### *Work done against Elasticity.*

Since the extension is proportional to the force, the whole work done will be the product of the extension and the mean value of the force; or if  $x$  is the extension and  $F$  the force,

$$x = eF,$$

$$\text{work} = \frac{1}{2} Fx = \frac{1}{2} eF^2 = \frac{1}{2} \frac{1}{e} x^2.$$

When the piece is inextensible, or  $e=0$ , then all the work applied at one end is transmitted to the other, and the frame may



be regarded as a machine whose efficiency is perfect. Hence the following

*Theorem.*—If  $p$  be the tension of the piece A due to a tension-unity between the points B and C, then an extension-unity taking place in A will bring B and C nearer by a distance  $p$ .

For let  $X$  be the tension and  $x$  the extension of A,  $Y$  the tension and  $y$  the extension of the line BC; then supposing all the other pieces inextensible, no work will be done except in stretching A, or

$$\frac{1}{2} Xx + \frac{1}{2} Yy = 0.$$

But  $X = pY$ , therefore  $y = -px$ , which was to be proved.

*Problem I.*—A tension F is applied between the points B and C of a frame which is simply stiff; to find the extension of the line joining D and E, all the pieces except A being inextensible, the extensibility of A being  $e$ .

Determine the tension in each piece due to unit tension between B and C, and let  $p$  be the tension in A due to this cause.

Determine also the tension in each piece due to unit tension between D and E, and let  $y$  be the tension in the piece A due to this cause.

Then the actual tension of A is  $Fp$ , and its extension is  $eFp$ , and the extension of the line DE due to this cause is  $-Fepq$  by the last theorem.

*Cor.*—If the other pieces of the frame are extensible, the complete value of the extension in DE due to a tension F in BC is

$$-F\Sigma(epq),$$

where  $\Sigma(epq)$  means the sum of the products of  $epq$ , which are to be found for each piece in the same way as they were found for A.

The extension of the line BC due to a tension F in BC itself will be

$$-F\Sigma(ep^2),$$

$\Sigma(ep^2)$  may therefore be called the resultant extensibility along BC.

*Problem II.*—A tension F is applied between B and C; to find the extension between D and E when the frame is not simply stiff, but has additional pieces R, S, T, &c. whose elasticities are known.

Let  $p$  and  $q$ , as before, be the tensions in the piece A due to unit tensions in BC and DE, and let  $r, s, t$ , &c. be the tensions in A due to unit tension in R, S, T, &c.; also let  $R, S, T$  be the tensions of R, S, T, and  $\rho, \sigma, \tau$  their extensibilities. Then the tension A

$$= Fp + Rr + Ss + Tt + \&c.;$$

the extension of A

$$= e(Fp + Rr + Ss + Tt + \&c.);$$

the extension of R

$$= -F\Sigma(epr) - R\Sigma(er^2) - S\Sigma(ers) - T\Sigma(ert) + \&c. = R\rho;$$

extension of S

$$= -F\Sigma(eps) - R\Sigma(ers) - S\Sigma(es^2) - T\Sigma(est) = S\sigma;$$

extension of T

$$= -F\Sigma(ept) - R\Sigma(ert) - S\Sigma(est) - T\Sigma(et^2) = T\tau;$$

also extension of DE

$$= -F\Sigma(epq) - R\Sigma(eqr) - S\Sigma(eqs) - T\Sigma(eqt) = x,$$

the extension required. Here we have as many equations to determine R, S, T, &c. as there are of these unknown quantities, and by the last equation we determine  $x$  the extension of DE from F the tension in BC.

Thus, if there is only one additional connexion R, we find

$$R = -F \frac{\Sigma(epr)}{\Sigma(er^2) + \rho},$$

and

$$x = -F \left\{ \Sigma(epq) + \frac{\Sigma(epr)\Sigma(eqr)}{\Sigma(er^2) + \rho} \right\}.$$

If there are two additional connexions R and S, with elasticities  $\rho$  and  $\sigma$ ,

$$x = -F$$

$$\frac{\Sigma e(r^2 + \rho)\Sigma e(s^2 + \sigma) - (\Sigma(ers))^2}{\left\{ \begin{array}{l} \Sigma(epr)\Sigma(ers)\Sigma(eqs) + \Sigma(eps)\Sigma(eqr)\Sigma(ers) + \Sigma(epq)\Sigma e(r^2 + \rho)\Sigma e(s^2 + \sigma) \\ - \Sigma(epr)\Sigma(eqr)\Sigma e(s^2 + \sigma) - \Sigma(eps)\Sigma(eqs)\Sigma e(r^2 + \rho) - \Sigma(epq)(\Sigma(ers))^2 \end{array} \right\}}$$

$$\left\{ \begin{array}{l} \Sigma(epr)\Sigma(ers)\Sigma(eqs) + \Sigma(eps)\Sigma(eqr)\Sigma(ers) + \Sigma(epq)\Sigma e(r^2 + \rho)\Sigma e(s^2 + \sigma) \\ - \Sigma(epr)\Sigma(eqr)\Sigma e(s^2 + \sigma) - \Sigma(eps)\Sigma(eqs)\Sigma e(r^2 + \rho) - \Sigma(epq)(\Sigma(ers))^2 \end{array} \right\}$$

The expressions for the extensibility, when there are many additional pieces, are of course very complicated.

It will be observed, however, that  $p$  and  $q$  always enter into the equations in the same way, so that we may establish the following general

*Theorem.*—The extension in BC, due to unity of tension along DE, is always equal to the extension in DE due to unity of tension in BC. Hence we have the following method of determining the displacement produced at any joint of a frame due to forces applied at other joints.

1st. Select as many pieces of the frame as are sufficient to render all its points stiff. Call the remaining pieces R, S, T, &c.

2nd. Find the tension on each piece due to unit of tension in the direction of the force proposed to be applied. Call this the value of  $p$  for each piece.

3rd. Find the tension on each piece due to unit of tension in

the direction of the displacement to be determined. Call this the value of  $q$  for each piece.

4th. Find the tension on each piece due to unit of tension along R, S, T, &c., the additional pieces of the frame. Call these the values of  $r$ ,  $s$ ,  $t$ , &c. for each piece.

5th. Find the extensibility of each piece and call it  $e$ , those of the additional pieces being  $\rho$ ,  $\sigma$ ,  $\tau$ , &c.

6th. R, S, T, &c. are to be determined from the equations

$$R\rho + R\Sigma(er^2) + S(ers) + T\Sigma(ert) + F\Sigma(epr) = 0,$$

$$S\sigma + R\Sigma(ers) + S(es^2) + T\Sigma(est) + F\Sigma(eps) = 0,$$

$$T\tau + R\Sigma(ert) + S(est) + T\Sigma(et^2) + F\Sigma(ept) = 0,$$

as many equations as there are quantities to be found.

7th.  $x$ , the extension required, is then found from the equation

$$x = -F\Sigma(epq) - R\Sigma(erq) - S\Sigma(eqs) - T\Sigma(eqt).$$

In structures acted on by weights in which we wish to determine the deflection at any point, we may regard the points of support as the extremities of pieces connecting the structure with the centre of the earth; and if the supports are capable of resisting a horizontal thrust, we must suppose them connected by a piece of equivalent elasticity. The deflection is then the shortening of a piece extending from the given point to the centre of the earth.

*Example.*—Thus in a triangular or Warren girder of length  $l$ , depth  $d$ , with a load  $W$  placed at distance  $a$  from one end, 0; to find the deflection at a point distant  $b$  from the same end, due to the yielding of a piece of the boom whose extensibility is  $e$ , distant  $x$  from the same end.

The pressure of the support at 0 =  $W \frac{l-a}{l}$ ; and if  $x$  is less than  $a$ , the force at  $x$  will be  $\frac{W}{dl} x(l-a)$ , or

$$p = \frac{x(l-a)}{dl}.$$

If  $x$  is greater than  $a$ ,

$$p = \frac{a(l-x)}{dl}.$$

Similarly, if  $x$  is less than  $b$ ,

$$q = \frac{x(l-b)}{dl};$$

but if  $x$  is greater than  $b$ ,

$$q = \frac{b(l-x)}{dl}.$$

The deflection due to  $x$  is therefore  $Wepq$ , where the proper values of  $p$  and  $q$  must be taken according to the relative position of  $a$ ,  $b$ , and  $x$ .

If  $a$ ,  $b$ ,  $l$ ,  $x$  represent the number of the respective pieces, reckoning from the beginning and calling the first joint 0, the second joint and the piece opposite 1, &c., and if  $L$  be the length of each piece, and the extensibility of each piece  $=e$ , then the deflection of  $b$  due to  $W$  at  $a$  will be, by summation of series,

$$= \frac{1}{6} WeL^2 \cdot \frac{a(l-b)}{d^2 l} \{2b(l-a) - (b-a)^2 + 1\}.$$

This is the deflection due to the yielding of all the horizontal pieces. The greater the number of pieces, the less is the importance of the last term.

Let the inclination of the pieces of the web be  $\alpha$ , then the force on a piece between 0 and  $a$  is  $W \frac{l-a}{l \sin \alpha}$ , or

$$p' = \frac{l-a}{l \sin \alpha} \text{ when } x < a,$$

and

$$p' = \frac{a}{l \sin \alpha} \text{ when } x > a.$$

Also

$$q' = \frac{l-b}{l \sin \alpha} \text{ when } x < b,$$

$$q' = \frac{b}{l \sin \alpha} \text{ when } x > b.$$

If  $e'$  be the extensibility of a piece of the web, we have to sum  $W \sum e' p' q'$  to get the deflection due to the yielding of the web,

$$= \frac{We'}{l^2 \sin^2 \alpha} a(l-b) \{l + 2(b-a)\}.$$

## LI. *Proceedings of Learned Societies.*

### ROYAL INSTITUTION OF GREAT BRITAIN.

Feb. 12, "ON the Synthesis of Organic Bodies." By J. Alfred Wanklyn, Esq., Professor of Chemistry, London Institution.

On this tray you will see a collection of well-known substances. Compare these substances with one another, and you will be struck with their dissimilarities. Some are solids and crystalline and brittle, others are liquids which are more fluid than water. Some are without colour; others are highly coloured, and are used for dyeing. Some



are sweet, others are bitter ; some have delightful perfumes, others have dreadful smells ; some are wholesome food, others the most powerful poisons known to man.

In spite of this wonderful diversity in their properties, all the specimens on this tray are compounds of carbon with a very few elements. Carbon, hydrogen, oxygen, and nitrogen are the only elements which occur in this collection of substances. Some of these substances contain carbon and hydrogen ; some contain carbon, hydrogen, and oxygen ; some, carbon, hydrogen, and nitrogen ; and some again contain carbon, hydrogen, oxygen, and nitrogen. But not one of the specimens on this tray contains anything besides these four elements.

There is no difficulty in resolving any one of these substances into its ultimate elements. This sugar, for example, on being heated to redness in a tube, leaves a black deposit which is carbon, whilst a liquid which is water distils over. If we were to electrolyze this liquid, we should obtain hydrogen and oxygen, and so we should exhibit carbon, hydrogen, and oxygen obtained from sugar. Again, instead of heating this sugar in the tube without allowing the air free access to it, we might burn it in excess of oxygen. If we were to do so, we should obtain carbonic acid and water ; and moreover all the carbon in the sugar would assume the form of carbonic acid, and all the hydrogen the form of water. So we can obtain carbon and hydrogen either in the free state or in the very common and well-known forms of combination as carbonic acid and water. Nitrogen, when it is present, can be made to assume the form of free nitrogen. For that purpose, all that is requisite is to heat the substance to redness with excess of oxygen, and to adopt certain precautions to avoid the production of oxide of nitrogen.

Thus the pulling to pieces of these substances on the tray is a matter of very little difficulty ; more than fifty years ago chemists could do that ; but how to put the pieces together again is a much more difficult task.

Sugar consists of 72 parts by weight of carbon, 11 parts of hydrogen, and 88 parts of oxygen. We may bring together carbon, hydrogen, and oxygen in these proportions, and shake them up together, or heat them or cool them, and yet we shall never get them to combine so as to form sugar. Alcohol consists of 24 parts of carbon, 6 parts of hydrogen, and 16 parts of oxygen, but no alcohol ever results from making such a mixture. Neither sugar nor alcohol can exist at the temperature to which it is requisite to raise our mixture of carbon, hydrogen, and oxygen in order to get chemical action to set in. At ordinary temperature the organic elements will not enter into combination ; whilst at high temperatures they combine, it is true, but yield comparatively very few compounds.

It was long after chemists had effected the analysis of organic bodies before they learnt how to effect the synthesis of even one of them ; and hence the belief sprung up that organic products, such as those on our tray, were intrinsically different from mineral products. Whilst stones, water, and the like were regarded as having their

ultimate particles held together by mere dead forces, sugar, alcohol, &c. were regarded as being held together by vital forces, as being, in short, in some subordinate way, alive.

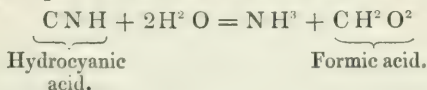
Now no more positive refutation of this notion can be imagined than the artificial construction of substances in every respect like those obtained from the animal and vegetable kingdoms; and hence some of the philosophical interest attached to the problem which forms the subject of this discourse.

The first definite example of the construction of an organic body from inorganic materials was given by Wöhler in 1828, when he made the organic base urea from cyanate of ammonia.

Let us trace the steps of this process. Cyanide of potassium—a body which can exist at a red heat [some cyanide of potassium was exhibited in the form of tabular masses which had been fused], and which can moreover be formed directly from its constituents (carbon, nitrogen, and potassium)—was oxidized by means of peroxide of manganese at a low red heat, and so cyanate of potash was obtained. The cyanate of potash was next converted into cyanate of ammonia by double decomposition with sulphate of ammonia. Thus cyanate of ammonia was produced from its elements by a process which, although indirect, still did not involve the action of either a plant or an animal. Cyanate of ammonia becomes urea, when its solution in water is simply evaporated to dryness.

It was curious that the first organic body to be constructed should have been a nitrogenous compound.

In 1831, three years after this important discovery of Wöhler's, formic acid—the first term of the fatty acid series—was obtained from inorganic materials by Pelouze. The process was this:—Hydrocyanic acid, a body capable of being obtained from inorganic materials, was heated either with strong alkalies or acids, and was so made to react upon the elements of water as follows:—



and yielded formic acid.

It does not appear that this research of Pelouze's attracted that attention which it deserved. This we must attribute to the circumstance, that at this period the position of formic acid in the organic series was not recognized.

The next step of importance in organic synthesis was taken by Kolbe in 1845. It was the synthesis of acetic acid, the second term of the fatty series. Kolbe's process was this:—Sulphide of carbon, obtained by the direct combination of carbon with sulphur at a red heat, was submitted to the action of chlorine at a red heat, by which means certain compounds of carbon and chlorine were obtained. One of the compounds,  $\text{C}^2\text{Cl}^4$ , was then acted upon by chlorine in the presence of water, and trichloroacetic acid resulted.

Having thus got trichloroacetic acid by thoroughly inorganic means, Kolbe availed himself of the observation which had been made by

Melsens—that treatment of trichloroacetic acid with potassium-amalgam and water converted it into acetic acid.

Kolbe was fully sensible of the scope and importance of his discovery. The following passage occurs in his paper, published in Liebig's *Annalen* for 1845 :—"From the foregoing observations we deduce the interesting fact that acetic acid, hitherto known only as a product of the oxidation of organic materials, can be built up by almost direct synthesis from its elements. Sulphide of carbon, chloride of carbon, and chlorine are the agents which, along with water, accomplish the transformation of carbon into acetic acid. If we could only transform acetic acid into alcohol, and out of the latter could obtain sugar and starch, then we should be enabled to build up these common vegetable principles, by the so-called artificial method, from their most ultimate elements." Thus it appears that Kolbe looked forward to the building up of organic bodies in general, and that he was quite alive to the fact that the synthesis of acetic acid completed the synthesis of the derivatives of acetic acid.

Among these derivatives may be enumerated acetone, the product of the destructive distillation of acetates; marsh-gas, obtained by distilling an acetate with a caustic alkali; ethylene, obtained by Bunsen, by heating kakodyle, which itself results by the action of arsenious acid upon an acetate. The electrolysis of acetic acid, which Kolbe accomplished a few years afterwards, yielded methyle and oxide of methyle, which latter, in its turn, could be transformed into any other methylic compound.

Marsh-gas was moreover prepared by Regnault, by treating  $\text{C Cl}_4$  with nascent hydrogen; and the common methylic compounds appear to have been produced by Dumas from marsh-gas, the chloride of methyle having been obtained by Dumas by the action of chlorine upon marsh-gas.

Before 1854, all the foregoing syntheses were fully completed, *i. e.* there was no step missing between the elements themselves and the most complex compound reached; but, in addition to these complete and definite syntheses, there had also been a good deal of building up of an incomplete or of a less definite character before 1854.

It was known in a general way, that organic bodies of tolerably simple composition sometimes gave complex products on destructive distillation. Thus alcohol was known to give naphthaline, benzole, and carbonic acid when it was pressed through a red-hot tube. Formiates were also known to yield hydrocarbons when they were subjected to destructive distillation. The precise dates of these different observations I cannot give; but hand-books of chemistry, published before 1854, contain a statement of the facts.

A few years after 1820, before Wöhler's celebrated Synthesis of Urea, a very remarkable instance of passage from a simpler to a more complex compound was given by Faraday and Hennel. This example is placed along with the indefinite syntheses, because it was generally disbelieved in by chemists, and only within the last few years, when it was confirmed by Berthelot, received their general assent. Faraday and Hennel found that olefiant gas was absorbed by sul-



phuric acid and gave sulphovinic acid, from which of course alcohol and the ethers might be procured. Liebig denied what Faraday and Hennel had asserted; and the latter did not insist upon the correctness of their work, and did not take the necessary steps for ensuring the reception of their results.

Shortly before 1854, a most capital addition to the art of organic synthesis was borrowed from the doctrine of the homologous series. I will endeavour to explain it.

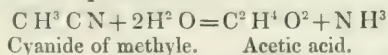
Organic bodies repeat themselves; thus common alcohol has a whole series of representatives, differing from it in formula by  $n \text{ CH}_2$ , but resembling it very closely in chemical functions. Alcohol, and these its representatives, constitute an homologous series. Every one of these representatives (homologues) of alcohol possesses a set of ethers and other derivatives, just as common alcohol possesses its ethers and derivatives. With certain limitations, it is true that whatever reaction can be accomplished with one alcohol can be accomplished with any other alcohol of the series.

Synthesis by series will then be easily understood by an example:— Suppose we obtain a building up by starting with common alcohol, we should infer that an analogous building up could be made by starting with any other alcohol of the series.

Here follows a Table of the homologous series of alcohols, and of the homologous acids which are related to them.

Methyle alcohol . . .	$\text{C}^1 \text{H}^1 \text{O}$	$\text{C}^1 \text{H}^2 \text{O}^2$ .	Formic acid.
Ethyle alcohol . . .	$\text{C}^2 \text{H}^6 \text{O}$	$\text{C}^2 \text{H}^4 \text{O}^2$ .	Acetic acid.
Propyle alcohol . . .	$\text{C}^3 \text{H}^8 \text{O}$	$\text{C}^3 \text{H}^6 \text{O}^2$ .	Propionic acid.
Tetryle alcohol . . .	$\text{C}^4 \text{H}^{10} \text{O}$	$\text{C}^4 \text{H}^8 \text{O}^2$ .	Butyric acid.
Amyle alcohol . . .	$\text{C}^5 \text{H}^{12} \text{O}$	$\text{C}^5 \text{H}^{10} \text{O}^2$ .	Valerianic acid.
Hexyle alcohol . . .	$\text{C}^6 \text{H}^{14} \text{O}$	$\text{C}^6 \text{H}^{12} \text{O}^2$ .	Caproic acid.
"    "    "    "    "    "			
Cetyle alcohol . . .	$\text{C}^{16} \text{H}^{34} \text{O}$	$\text{C}^{16} \text{H}^{32} \text{O}^2$ .	Palmitic acid.
"    "    "    "    "    "			
Ceryle alcohol . . .	$\text{C}^{27} \text{H}^{56} \text{O}$	$\text{C}^{27} \text{H}^{54} \text{O}^2$ .	Cerotic acid.

A good example of synthesis by series was furnished by Frankland and Kolbe, who showed that various cyanides of the alcohol radicals yield the next higher acid in the series when they are digested with an alcoholic solution of potash, thus:—



The effect of the alkali is to cause decomposition of water by means of the cyanide; and the reaction very closely resembles Pelouze's, of which mention has already been made.

By means of this synthesis, which is general to the whole series, chemists acquired a method of ascending from any given alcohol to the acid belonging to next higher alcohol. It will be evident, however, that this step, important though it was, did not suffice to enable chemists to march regularly up the ladder. The step from acetic acid to alcohol—from an acid to an alcohol of the same carbon-condensation—was wanting.

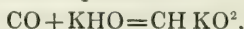


This synthesis by series was an incomplete synthesis, there was a gap requiring to be filled up, in order that the regular march might be made up the vinic series.

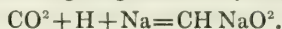
From the foregoing, it will be seen that by the year 1854 very considerable progress had been made in the building up of organic bodies from their ultimate elements.

We now pass on to the consideration of the period comprising the last ten years, from 1854 up to the present time.

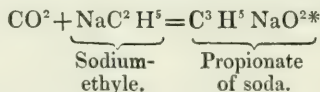
During this period we have had new methods of accomplishing some of the syntheses which had been effected previously. Thus formic acid, which, as we have seen, had been formed from inorganic materials so long ago as 1831, was built up by Berthelot by means of carbonic oxide and caustic potash,



and again by Kolbe by using carbonic acid, moisture, and sodium (the moisture and sodium giving nascent hydrogen),



Again, also, the passage from an alcohol to the next higher acid was repeated. Carbonic acid and a compound of an alcohol radical with an alkali-metal coalesced and formed a salt of a fatty acid, thus :—

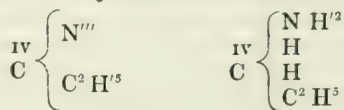


Still these reactions, however interesting they might be, were not new syntheses ; they were only new methods of effecting old syntheses.

The great problem, how to step from one alcohol to that next above it, has received a general solution from Mendius. Mendius used cyanogen compounds—those hydrocyanic ethers which had already done such good service to organic synthesis—and exposed them to the action of nascent hydrogen, and so obtained amides of alcohol radicals higher than the alcohol radicals started from. The reaction bears a close similarity to the one which takes place between the cyanides and alcoholic solution of potash, and which, as will be remembered, enabled us to pass from the alcohol to the acid next above.

Here is a scheme to show the parallel :—

*Transformation to Amide.*



\* The experiment was shown ; and the great evolution of heat which took place on bringing carbonic acid into contact with sodium-ethyle was apparent.

*Transformation to Acid.*



In the one case nitrogen is replaced by  $\text{NH}^2$  and H, H; and in the other by  $\text{HO}'$  and  $\text{O}''$ .

Mendius was able to commence even with hydrocyanic acid. The steps in his synthesis are these :—

*Hydrocyanic Acid to Methylamine.*



Methylamine, by means of nitrous acid to methyl-alcohol : methyl-alcohol to cyanide of methyle, well-known processes being employed to effect this.

*Cyanide of Methyle to Ethylamine.*

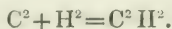


From ethylamine it is easy to get cyanide of ethyle, from which, by a third repetition, we arrive at the propylic stage :—



Thus the vinic series may be ascended; thus there is reason to think we may begin with so simple a body as prussic acid, and step by step proceed from one alcohol to the next above it, until we reach the fats and the waxes. There are other methods of effecting the synthesis of the alcohol series, but none of them seem to be so complete and satisfactory as this. Berthelot has obtained alcohols by adding the elements of water to the olefines, and some of the olefines he has obtained by the destructive distillation of formiates; but it is an open question, how many olefines can be got by heating the formiates? And, at any rate, there is no precision in the preparation of olefines from formiates.

A very neat and beautiful way of preparing one olefine, viz. common olefiant gas, is, however, due to Berthelot. He exposes charcoal to the action of hydrogen at a very high temperature—the temperature of the electric arc; and then union takes place, resulting in the formation of acetylene :—



Acetylene exposed to the action of nascent hydrogen in an alkaline liquid gives olefiant gas :—



Friedel and Wurtz have converted aldehydes and ketones into alcohols by the action of nascent hydrogen, and thence there arises another method of ascending the vinic series; and besides there are a number of other reactions which are capable of more or less general employment for the purpose of building up the alcoholic series, but which we have not time to particularize.

The alcohols having been got, many other important organic compounds follow, and there is good reason for believing that with the progress of the science all will be derived from them, so that the series of the alcohols will constitute a kind of backbone to organic chemistry.

Most modern organic researches are capable of being looked at from a synthetical aspect, for they generally disclose how to devise some organic bodies from compounds which themselves either are, or will be, capable of complete synthesis. Glycerine, the base of the fats, has been derived from the propylic series, having been obtained by Wurtz by a somewhat circuitous process from propylene—the olefine of that series.

The sugars have not been as yet *unequivocally* produced, but they will be, for their connexion with the hexylic series is now placed beyond a doubt. The production of glycerides from glycerine and fatty acids is the proof that the natural fats are within our grasp. The aromatic series, with its many derivatives, among which may be mentioned the wonderful aniline dyes which rival those got more immediately from the animal and vegetable kingdoms, becomes accessible to synthesis through common alcohol, which on being heated to redness gives benzole and carbolic acid—members of the aromatic series.

Wurtz's compound ammonias, and, above all, the immense and wonderful development of the class of compound ammonias arising from the labours of Hofmann, are the pledge that the natural alkaloids—quinine, morphine, strychnine, and their congeners—will one day be within our reach.

Glycocoll, produced by Perkin and Duppa from acetic acid, and the bases of the juice of flesh, which have been recently formed by Volhardt and Hofmann, assure us that albumen—that essential ingredient of our food—will not elude us.

Why should those medicines and foods which we find in nature be the most useful which are possible? Would it not rather be strange if they were?

Hereafter, perhaps, medicines as much more potent than quinine as quinine is than the extracts of the commonest herb that grows wild may be the produce of our laboratories.

### *LII. Intelligence and Miscellaneous Articles.*

FURTHER REMARKS ON THE TELESCOPIC APPEARANCE OF THE EXTERIOR ENVELOPE OF THE SUN, AND OF ITS SPOTS. BY THE REV. W. R. DAWES.

**B**EFORE proceeding to consider the character and cause of the solar spots, it may be desirable to recur to some points connected with the telescopic appearance of the general surface, in addition to the remarks contained in my former paper (p. 156) on the subject.

In speaking of the less luminous interstices between the intensely luminous masses forming together the general surface of the sun, I

have used the term *pores* for the extremely minute openings found in them. This I adopted from Sir John Herschel's description of the phenomena. It may, however, be necessary to guard against the supposition that these pores are always or even usually quite *round*. When best seen, under circumstances which are unfortunately of very rare occurrence, they sometimes have rather the appearance of very small *fissures*, such as might be caused by several exceedingly small dots running together into one short dark line, which is never *black*, but at most of a *dark grey*. I have been strongly impressed with the conviction that their *visibility* varies greatly at different times, even under circumstances too similar to account satisfactorily for the difference. Indeed I have on some occasions been able to find none, or scarcely any; while on others, not remarkably more favourable, they have come out by pretty steady views in a sufficiently certain and positive way.

Perhaps this variation in their visibility may be connected with another fact to which I should wish to draw special attention, namely, that *the proportion of the area of the less luminous spaces (in which the pores are sometimes found) to that of the most luminous masses is subject to very considerable change*. Though they are at no time very small or narrow, yet of late they have struck me as occupying a decidedly greater space than I used to assign to them. If such changes really exist, and are also great, they would seem necessarily to produce differences in the quantity of solar light, and might thus place our sun among the slightly variable stars, and also afford a plausible way of accounting for the variation in others. I have indulged the hope of being able to give a more decided opinion as to the actual proportion between the masses of different luminosity; but I can only say that at the present time my impression is that the area occupied by the less luminous masses is even *greater* than that of the brighter ones, but that the proportion in different parts of the sun is by no means always the same at the same time. These conclusions may, however, be considerably modified by better and more continuous observations (with this special object in view) than I have been able to procure, especially during the last eight months. Might it not be reasonably expected that *photography* would here step in most efficiently to our assistance? As an approximation to what is wanting, I may refer to the very remarkable print of the sun's surface from an electro-copper block obtained from the original negative taken by Mr. Warren De la Rue. The dark interstices in that plate seem to represent the less luminous portions of the surface, which may possess far less photographic power. How such a picture can be reconciled with the idea of an interlacing of an infinite number of willow-leaf-shaped masses of equal brightness I am at a loss to conceive. The brighter parts are in that picture, as I have always seen them, of all imaginable forms, and could not possibly be produced by any such interlacing.

Another feature of these masses of different brightness may here be adverted to. They certainly give the impression of an undulating surface, much like that of an orange, or of a fine head of cauliflower



when becoming a trifle loose. The brightest masses seem to be higher and therefore thicker than the others; the brightness of the lower and thinner parts being modified by the decidedly inferior luminosity of the second or penumbral stratum, which is, as it were, partially seen through it. This idea would seem to be greatly strengthened by the fact that the remarkably bright streaks called *faculæ* are undoubtedly *ridges*, being elevated to a considerable extent above the general surface,—of which I have had ocular demonstration on more than one occasion by their actual projection at the limb of the sun, like an irregular range of hills at the edge of the moon, as well as by their frequently, either partially or entirely, concealing openings which have soon been brought into view by the sun's rotation.

However plausible this conclusion may appear, there are two facts which strongly militate against it. One is, that the less luminous masses are seen *to the very edge of the sun*, both near the equator and at the poles, except when, near the equatorial edge, they are concealed by obvious *faculæ*. This seems to be quite inconsistent with their being to any considerable extent depressed below the brighter masses, sufficiently so at least to produce so great a difference in their brightness. The second fact is, that the boundaries between the two kinds of masses referred to are *too abrupt*, and the change of brightness too sudden, to allow of anything like gentle undulations. The sides of the brighter portions must be almost precipitous to produce the views we obtain of them under the finest circumstances.

From these facts I have been led to the conclusion that the masses of inferior brightness, though self-luminous like the brighter, are *so far different in their nature that the two do not readily mingle together*, and that it is only at their confines that they do so at all; and also that their surfaces are on the whole at the *same level*—the principal exceptions being found in the *faculæ*, which are formed entirely of the brighter portions. Those which are less luminous may on that account be more transparent, and perhaps also less dense, and therefore more readily permitting the escape of some non-inflammable gas producing the pores or fissures occasionally found in them.

The apparent *mottling* on the sun's surface is obviously produced by the juxtaposition of the more and less luminous masses or clouds; and I must confess myself quite unable to perceive how such a condition can be compatible with a pretty uniform distribution of luminous masses of any one shape or degree of brightness. The fine mottling is a certain fact, easily verified with a very moderate amount of instrumental aid, and subject to no suspicion of being an optical deception. It certainly seems to me, therefore, that a proper degree of caution may well make us hesitate to admit as a fact any appearance obviously incompatible with it, only rarely seen by very few, and never once perceived by others during years of the most careful scrutiny under the most favourable circumstances and with optical means perfectly adapted to the purpose.

We may now proceed to consider the *cause and mode of formation of the different kinds of solar spots*. Any one accustomed to observe

them, even in the most cursory way, must have been struck, not only with the endless variety of their forms, but also with an essential difference in their character. For our present purpose they may perhaps be most suitably divided into two classes, the *profound* and the *superficial*.

1. The *profound*.—In this class I should include those which give evidence of involving all the visible envelopes—the disturbance being observable through them all, and down to what appears to be the body of the sun itself. Among the *envelopes* I do not include any exterior to the photosphere, but restrict the term to the *three* which I have already referred to, but will here more particularly specify.

The *first* in order from the body of the sun outwards is what, in my 'Description of a new Solar Eyepiece,' I pointed out as being really an envelope, and not the body of the sun, as it had been previously accounted and as such had been generally called the *nucleus* when it occupied the middle of a spot. By using a field of view sufficiently small to exclude even the *penumbra* of pretty large spots, and using as light a glass as the eye can comfortably bear, a mottled appearance is perceived in this stratum, the degree of darkness being by no means uniform, and suggesting the idea that the surface we see is far from level—the lighter parts being probably the most elevated, and feebly reflecting the light received from the self-luminous strata above it. To this lowest envelope I applied the term *cloudy stratum*, forming the *umbra* of a spot. In all spots which are tolerably symmetrical, and large enough to admit of accurate scrutiny, the *umbra* will be found to be perforated near its centre by a *perfectly black hole* which I regard as the true *nucleus*. The form of the *umbra* usually approximates to that of the *nucleus*, being nearly round when the *nucleus* is so; and when the *nucleus* is much elongated, or when some part of it greatly varies in shape from the rest, the general form of the *umbra* is almost constantly found to partake of a similar irregularity. A very remarkable feature of this cloudy *umbra* is, that at the edge next to the *nucleus* it is usually decidedly less dark than elsewhere, giving the impression of being there more elevated or heaped up. This is especially the case with spots which have only recently broken out, or are in the process of enlargement. On the contrary, when a spot is closing up, this feature disappears. Occasionally some very small bright specks are visible on this stratum; but I believe they do not belong to it at all, but are minute portions of one of the self-luminous strata above it.

The *second* or *middle* envelope constitutes the *penumbra* which usually attends all spots of considerable size and symmetrical forms. It possesses, I think, all the characters of self-luminosity, though its brightness is far inferior to that of the outer envelope. In most cases lines of much greater brightness are projected upon it, which obviously belong to the outer one, or photosphere. But occasionally some parts of the *penumbra*, especially where it is very broad, are decidedly brighter than the rest, and these are rarely, if ever, overlaid with the bright lines from the photosphere. They give me the impression of belonging, not to the penumbral stratum at all, but to

the *less* luminous portions of the photosphere projected on to the penumbral stratum. At the interior edge of the penumbra we generally find an appearance (especially when spots are enlarging) very similar to what I have noticed above as being found at the interior edge of the umbra—namely, an increase of brightness as compared with the rest of the penumbra—the light decreasing from its inner to its outer edge, where it is almost always perceptibly less than in any other part. That this is not the effect of contrast with the adjacent far brighter photosphere is proved by excluding that stratum from a small field of view. The impression is the same as in the case of the umbra—that the inner edge of the penumbra is heaped up, and thicker than any other part of it.

The *third* envelope, or *photosphere*, affords the same kind of evidence of being heaped up, or rolled back on itself, at its interior edge adjoining the outer edge of the penumbra. As I have stated above, it is not unfrequently *seen* to be so when spots of considerable size are near the sun's limb. The elongation of the brighter self-luminous masses, as seen projected on the penumbra, and on some special occasions at its outer edge, in the formation of luminous bridges, has been adverted to in my paper recently presented to the Society.

These remarks on the peculiar characteristics of the three envelopes will suffice to show on what evidence my conclusions rest respecting the manner in which these *profound* spots are formed, and which may be thus described.

An immense volume of some non-inflammable gas, discharged with prodigious force from the body of the sun by volcanic or some similar agency, bursts through the *cloudy stratum*, rolling back on all sides the displaced portion of that stratum, and producing the heaped-up appearance at its inner and lighter edge. The *black hole* produced in the stratum by this volcanic eruption forms the *nucleus* of the spot.

Having passed through the cloudy stratum, the evolved gas comes within the influence of the heating power of the self-luminous penumbral stratum; and being greatly expanded thereby, its increased volume removes a far larger area of this second stratum than of the first, thus laying bare a considerable portion of the upper surface of the cloudy stratum, and producing the *umbra* of the spot. Here, too, the rolling back of the removed portion causes a heaped-up and brighter appearance at the inner edge of the penumbra. Being still further heated and expanded by approaching the photosphere, a similar effect is produced upon this upper stratum, but to a far greater extent; and a much larger portion of the photosphere is thrown off on all sides, which being, as before, rolled back upon the rest, gives the appearance of a heaping up of the luminous masses at the exterior edge of the spot.

Such is the process, or something like it, by which, as I have been led to conclude, these "profound" spots are formed. And it may here be remarked, that an additional argument for the *smaller density* of the *less* luminous masses seen in the photosphere (and before referred to) is found in the fact that these seem to be almost entirely



removed from above the penumbra; leaving in general only the elongated forms of the *brightest* self-luminous clouds of the photosphere, which, converging towards the nucleus, are seen projected on the penumbra, and extending often beyond its interior edge on to the umbra, like bright straws, as I have previously described them.

The *rotatory* motion of a *profound* spot may be produced by the exploded gas having acquired a whirlwind sort of action, and thus carrying round the parts of the different strata affected by it in the same direction. But though I have observed this motion with certainty on two or three occasions, I believe it to be exceedingly rare—at least in this class of spots.

2. The *superficial* spots.—These appear, from the general tenour of my observations, to be almost always produced by convulsions of some kind in the photosphere itself, or at a small depth below it. But, from the extraordinary variety of the effects, I confess that I am not prepared to add anything to the suggestions already advanced as to the character of those convulsions, or the means by which they may be produced. Many of the spots which seem to be thus formed are remarkable for the immense extent of the *abrasions*, so to speak, of the photosphere, leaving large areas of the penumbral stratum exposed, but with none, or scarcely any, of it removed so as to show the cloudy stratum and thus to produce any *umbra* at all. In these cases, the exposed penumbral stratum has rarely any bright lines of the photosphere projected upon it. Another remarkable fact is, that at some times, for months together, there is a tendency to the formation of small superficial spots in considerable number (as from 8 to 12), arranged nearly in an *elliptic* form, the longer axis being nearly parallel to the sun's equator. I have occasionally observed more than one of these curious elliptic outbreaks in different parts of the sun at the same time. And there is almost universally this singular feature attending the spots thus arranged: *the penumbra of each is expanded outwards from the centre of the ellipse*. Not unfrequently there is in such cases nothing but the penumbra exposed, no umbra being visible: and even when a small umbra is exposed, the interior edge of each spot is formed by the photosphere, no penumbra appearing on that side. The whole appearance gives the idea of a prodigious and widely extended force emanating from the centre of the ellipse *at a level but little below the photosphere*, and acting so obliquely as to disturb that stratum only at great distances from the centre.

P.S.—I am indebted to the kindness of Mr. Birt for some extracts from an elaborate paper by Sir William Herschel, with which I was previously unacquainted. It forms a part of vol. xci. of the *Philosophical Transactions*, and was read on the 16th of April 1801. The observations of several-years are referred to in it; and some of them are so remarkable, and so much to the point on the subject of the telescopic appearance of the general surface of the sun, that I think it will be highly desirable to quote a few passages most clearly expressing the conclusions to which that admirable observer had arrived.



On page 304 of vol. xci. he states that he uses the term *self-luminous clouds* "as a general expression for all phenomena of the sun, in what shape soever they may appear, that shine by their own light."

On page 287, referring to the "corrugations" all over the surface, which he considered to consist of elevations and depressions, he quotes some of his observations as follows:—

"Dec. 22, 1799. The whole disc of the sun is strongly indented.

"Dec. 23, 1799. The corrugations have a mottled appearance. I see the figure of the dark and bright places. Many of the dark places are not round, but a little extended in different directions, and appear to be lower than the bright places.

"Dec. 27, 1799. On examining some of the largest corrugations with a high magnifying power, I see plainly that the less bright parts or indentations are small openings, and that those dark places which are the coarsest show the opaque surface of the sun the best."

In another place he remarks, "The whole disc is very much marked with roughness like an orange. Some of the lowest parts of the inequalities are blackish."

"Jan. 31, 1800. The indentations are very uniform, but not round. It seems they admit of every possible shape.

"Feb. 4, 1800. The points, or rather darker-coloured places in the punctulations, were of different figures, few of them being round.

"Feb. 18, 1800. Among all the corrugations, I could hardly perceive any that were round. They were of all shapes, chiefly lengthened.

"Jan. 15, 1801. The low places of corrugations do not contain punctures, but seem to be irregularly shaped places of less luminous matter than the borders which inclose them."

On these passages it may be remarked that the decided mention of pores or punctures on some occasions, and the statement on others that there were none to be seen, strongly supports the conclusion I have arrived at, that the visibility of these minute dark points is subject to great variation.

Though Sir William seems to have entertained no doubt that the less luminous portions of the photosphere were considerably depressed below the brighter parts, yet he states that they were visible in every part of the disc, and in one place observes, under date Dec. 20, 1794, "I can follow the indentations from the centre up to the margin of the sun; but it requires great attention, as, on account of the sphericity of the disc, they become gradually less conspicuous the nearer we go to the circumference. I saw them equally well at the north pole of the sun."

As I have before remarked, the depression must be extremely small for this to be the case; and I should be far more inclined to attribute the increased difficulty in making out the spaces of different luminosity to the great fore-shortening of both near the sun's edge, though they might be on the same level.—*From the Monthly Notices of the Royal Astronomical Society, January 8, 1864.*

## ON THE HYPOTHESIS OF MOLECULAR VORTICES.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

As the article on Thermo-dynamics in the North British Review is perhaps the most complete history of that science which has yet appeared, and is written with a scientific precision which is unusual in journals not specially devoted to science, I wish to correct an oversight that the reviewer has committed in describing the "Hypothesis of Molecular Vortices," or "Centrifugal Theory of Elasticity," as proposed by me in 1849\*. He speaks of atmospheres of æther surrounding nuclei of ordinary matter; whereas in the hypothesis as I put it forward, the nuclei perform the functions of æther, and the atmospheres those of ordinary matter. Radiance is supposed to consist in oscillations of the nuclei, transmitted in waves by means of the forces which they exert on each other at a distance; and thermometric heat is supposed to consist in an agitation of the atmospheres, producing outward pressure according to the known laws of centrifugal force. Emission of radiance takes place when the atmospheres whirl faster than the nuclei oscillate, so that the nuclei are undergoing acceleration, and the atmospheres retardation; absorption of radiance takes place when the nuclei oscillate faster than the atmospheres whirl, so that the nuclei are undergoing retardation, and the atmospheres acceleration. In perfect gases, the nuclei oscillate with little impediment from the atmospheres, and the transmission of radiance is rapid; in substances in a more dense condition, each nucleus is, as it were, loaded with a part of its atmosphere (like a pendulum in a resisting medium), and the transmission of radiance is slower. It is this peculiar view of the respective functions of the nuclei and the atmospheres that constitutes the main distinction between the hypothesis put forth by me, and other hypotheses involving atomic nuclei and atmospheres (as that of Mossotti), or accounting for the phenomena of heat by molecular motions (as that of Mr. Herapath).

Of course a mechanical hypothesis does not form an *indispensable* part of thermo-dynamics, more than of any other physical science; but if a hypothetical theory of thermo-dynamics is to be used, it appears to me that its fundamental principles must be such as I have described.

I am, Gentlemen,

Your most obedient Servant,

Glasgow, March 1, 1864.

W. J. MACQUORN RANKINE.

## ON THE DEFLECTION OF THE MAGNETIC NEEDLE BY THE SECONDARY CURRENTS OF THE LEYDEN BATTERY. BY P. RIESS.

The author obtained this deflection, which hitherto had not been possible, by means of an apparatus which he had used before, and to the construction of which an experiment of M. Gaugain had led.

\* Transactions of the Royal Society of Edinburgh, 1850-51.

The apparatus, which is called the "electrical valve," and which is analogous in construction to the apparatus known as the "electrical egg," consists of a small hollow glass cylinder of about the size of an ordinary drinking-glass, and closed below by a metal cap by which it can be screwed on the air-pump, while the top is closed by a tightly-fitting glass plate. In this plate there is a platinum wire which terminates flush with the lower surface of the plate, and externally can be joined to any conduction arrangement. In the inside of the cylinder there is a brass disc 25 millims. in diameter, supported on a brass stem, parallel to the covering plate, and at a distance of 2·3 millims. from it. When the valve, in which the air is rarefied to 1 or 2 lines of mercury, is introduced into the wire of the secondary current of the battery, the deflection of the needle of a multiplier interposed in the conduction ensues, in the one direction or the other, according to the position of the valve. The needle is deflected in the direction of a current, which has the same direction as the principal current when it proceeds from the disc, and in the opposite direction when it first meets the point. From this the following convenient rule is obtained :—*By means of the electrical valve, and in any position, the secondary current of the Leyden jar deflects a magnetic needle in the direction of a current proceeding from the disc to the point of the valve.*

First, a multiplier with double needle was used ; and as this was unfit for comparative experiments, a galvanometer was substituted in which a steel mirror suspended by a cocoon-thread was deflected by an adjacent coil of wire. The deflection by a feeble secondary current was four times as great as that produced by the principal one, and was not changed by the use of two valves. Experiments which the author had previously made on the simultaneous magnetization of three needles were repeated by inserting the valve in the lateral circuit. The magnetization produced by the secondary current was in the direction denoted by the deflection of the needle ; and the influence which the secondary exercises was perceptible when it was used in conjunction with the primary current for magnetizing a needle. The secondary current alone affords the easiest and most powerful means of imparting magnetism to a needle by means of the Leyden battery.

In a battery-circuit which, either by its great length or a special arrangement, affords no opportunity for developing a powerful secondary current, the valve is without action upon the magnetic deflection, but not so as regards the magnetization of the needle. This is strongest when the discharge proceeds from the disc to the point of the valve, thus confirming earlier experiments on the heating of the circuit.

The currents of higher order, which were investigated as far as the fifth order, deflected the magnetic needle by the same rule as that for the secondary current. The experiment is easy and certain in all currents ; for it was invariably successful, provided the pressure in the valve was not less than 2 lines, and not more than 5 inches of mercury. By an individual deflection experiment, a current of higher



order could not be distinguished from the current which produced it, or from that which it had produced; but there was a difference when a series of observations were made by gradually altering the pressure in the valve. The experiments gave the following rule:—If the secondary current in the same direction as the principal one first starts from the disc, the deflections slowly diminish and retain the same direction as currents of even order, if the pressure in the valve is increased; a rapid decrease and change in direction when the pressure is increased are characteristic of currents of uneven order. If the secondary current first starts from the point of the valve, the rule prevails, changing both kinds of currents.

The designation "electrical valve" might lead to the assumption that the instrument only transmits those currents, of whatever density and whatever origin, which follow a given direction from the disc to the point of the valve. The author adverts to the marked opposition in which all his experiments stand to the first experiments made with the valve. M. Gaugain has shown that the opening currents of the magneto-induction apparatus only deflect the magnetic needle when they proceed from the point to the disc of the valve. The valve might perhaps be a means of investigating the current of the induction apparatus, the most complicated of all electrical currents.

The magnetic deflection by the extra current in the wire itself which discharges the battery furnishes a surprising experiment, which M. Feddersen has described, but, in explaining it, has entirely overlooked the extra current. Two coils of wire are used as galvanometers, between which the magnetic needle oscillates. The coils are joined with each other at opposite ends, and from both wires connexions are led to the coatings of the battery. Hence the discharge divides into the two branches formed by the battery, and traverses them in opposite directions: the mirror can only show the difference of the deflections which the coils would individually have imparted. By moving one coil, the mirror is made motionless when the discharge passes. If now a valve is brought into each coil, and the valves have opposite directions as regards the discharge, a deflection of the mirror is obtained, and of any magnitude by modifying the charge of the battery, and, by the position of the valve, of any direction. The author altered the experiment by uniting the coils at similar ends, but only left one coil near the mirror, the other being at such a distance that it was without direct action on the deflection. When a deflection of the needle was produced by a discharge through the outer metallic circuit, by repeating the experiment and introducing two valves into the branches a twenty-fold deflection was obtained in any direction. The direction could be predicted by means of the above rule. The mirror is deflected by a secondary current which arises from two sources, the two coils of wire. One source, the coil distant from the mirror, was omitted and replaced by a short platinum wire of the same conducting value: the deflection of the mirror with an entire metallic circuit was the same as before, but, after introducing both valves, much less than in the absence of both coils. These



experiments show in a very striking manner the occurrence of the extra current in the circuit of the battery itself, and are not less conclusive than previous experiments of the author on the heating of the branches.—*Monatsbericht der Berliner Akademie* November 1863.

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NEW BRITISH MINERAL.

*To Dr. Francis, F.L.S. &c.*

DEAR SIR,

British Museum, March 16, 1864.

Would you do me the favour to find a corner in the April Number of the Philosophical Magazine for the announcement that I have got a new mineral from Cornwall, prismatic in crystalline form, and consisting probably of a basic sulphate of copper insoluble in water? It occurs in minute but brilliant crystals; and, in fine masses of the richest blue colour, it forms a thick incrustation upon a tender killas.

It came to the Museum from Mr. Talling of Lostwithiel, and I shall give a more exact description of it in an early Number of the Philosophical Magazine.

I am, dear Sir,

Truly yours,

—————  
N. S. MASKELYNE.

NEW INVESTIGATION ON THE POSITION OF THE OPTIC CENTRE OF THE EYE, AND DETERMINATION OF THE REFRACTIVE VALUES OF THE DIFFERENT MEDIA. BY M. GIRARD-TEULON.

I. By means of experiments, the object of which recalls those of Haldat, but in which I have modified (1) the direction of the light, to which, by reflexion from a plane mirror at an angle of  $45^\circ$ , I imparted an upward vertical direction, and (2) the method of observation, inasmuch as I examined the images formed on the posterior surface by means of the microscope, I obtained the following results.

1. Measuring the distance of the images at the anterior surface of the cornea for parallel rays, and then their distance when the object was brought within two inches of the eye, that is, at the two extreme limits of accommodation, I found, in opposition to the facts announced by Haldat, that during this motion of the object from the horizon to two inches from the eye the place of the images receded—

In the ox, about 6 millims; in the sheep, about 4 millims.; in the pig, about 3 millims.; in the human subject, about 2.5 to 3 millims.

2. Repeating these experiments with the eye deprived of cornea and of aqueous humour, the results were virtually the same. It appeared well established that the rays which fall on the crystalline in the air, and those proceeding from the same and which meet it after having undergone the refracting effect of the aqueous humour and of the cornea, form a focus at the same distance from the posterior face of the crystalline. (This fact had been already announced by Haldat.)

3. The crystalline being placed isolated in the air, the differences,

always real, between the length of the principal and of the conjugate focus of a point situated at two inches from the lens are only

About 1 millim. in the ox and sheep,  $\frac{1}{2}$  millim. in the pig,  $\frac{1}{3}$  to  $\frac{1}{2}$  millim. in the human subject.

Hence, to pass from distant vision to the vision of objects of 2 inches' distance, the eye needs an apparatus which, in the case of man, makes the interior conjugate focus traverse a distance of 2.5 to 3 millims. between parallelism of the incident rays and the divergence corresponding to 2 inches.

II. The optic centre of the entire eye, and even that of the crystalline isolated in the air, are behind the posterior face of the crystalline. In the eye of the albino rabbit, the only one in which the position can be accurately determined, the optic centre is exactly in the centre of figure and of motion of the globe.

A decisive physiological experiment shows the exactitude of this coincidence in the human eye at the time of the regular exercise of sight. The pupil is dilated by means of atropine; then by means of a fixed binocular ophthalmoscope, the *sharp* image of the flame of a candle is observed on the choroid by Knapp's method. The subject retaining his head quite fixed, his eye is caused to make regular and slow motions from one angle of the orbit to the other.

During these movements the image neither varies in magnitude nor in position, as can be established by a micrometer placed in the very point occupied by the inverted image. It follows evidently from this, that the optic centre coincides with the centre of motion of the globe.

This experiment further gives rise to the following accessory observation: if the narcotic used is weak enough not to have extinguished or materially impeded the accommodation, and the subject is led to make an effort directing his attention to a distant point in the same direction, as in Crammer's experiment, the image of the lamp, at first quite definite and sharp in its contours, suddenly becomes extended, confused, and badly defined. The change in the dioptric state during the adaptation of the eye is thus demonstrated once more.

The situation of the optic centre of the eye in the very centre of its rotation, behind and beyond the crystalline, evidently renders illusory the skilful calculations on which Listing bases the construction of his schematic eye—illusory, not from the mathematical point of view, but from the application to complex organic elements of the beautiful formulæ established by Gauss for homogeneous optical elements.

III. Referring the elements of the schematic eye to the real position of the optic centre, we shall represent the schematic eye in one or other of the two following manners, according as we want to approach more or less to the arrangements of the real eye.

The most simple combination would be a sphere transparent in front, terminating a homogeneous refringent medium, whose index of refraction is 2, and whose diameter is 23 millims.

The focus of such a system is at the extremity of its diameter, and the optic centre in the centre of the sphere.

To be closer to the real arrangement presented by nature, the index of refraction of this sphere, 23 millims. in diameter, would be 1.34; that being admitted, a condensing lens would be fastened upon it slightly projecting from it (the cornea), and representing a focal distance of  $\frac{1}{30}$  millim., in the medium 1.34.

Secondly, in the optical centre of the apparatus, a second condensing lens would be suspended of the same focal power,  $\frac{1}{30}$  millim. Their sum,

$$\frac{1}{18:50} + \frac{1}{30} = \frac{1}{11:50},$$

represents a lens of 11.50 focal length, as is necessary, in fact, to give clear images on the retinic screen.—*Comptes Rendus*, February 22, 1864.

ON THE GEOMETRY OF MY EIGHTH DECIMAL APPROXIMATION TO  
THE CIRCULAR CIRCUMFERENCE (PHIL. MAG. JAN. (SUPPL.),  
FEB., APRIL 1863). BY S. M. DRACH, F.R.A.S.

In the above-quoted papers I showed that

$$3.14159265 = \frac{21}{20}(3,000,000 - 8007) = \frac{63}{20}(997331 = 1000^2 - 2669),$$

and  $2669 = 50^2 + 13^2 = 35^2 + 38^2$ . This simple approach arose from successively considering

$$3 \cdot 15, 3 \cdot 15 - \cdot 00840 = 3 \cdot 14160, 3 \cdot 15 - \cdot 00840 - \cdot 00000735;$$

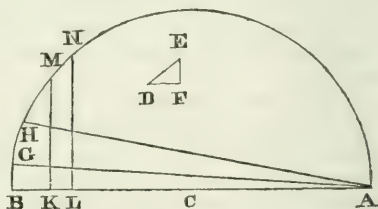
where 315, 840, 735=3, 8, 7 times 105. In the annexed diagram let diameter AB=1000, and in the auxiliary right-angled triangle DF=50 (or 38), and FE=13 (or 35); whence

$$DE = \sqrt{2669} = \text{chord } BG :$$

supplementary chord

$$GA = \sqrt{997331}.$$

Make  $B_K = \text{one-eighth } B_A$ ,

$$B L = \text{one-sixth } B A; \quad K M,$$


LN  $\perp$  to BA to circle;  $\therefore$  KM<sup>2</sup>:LN<sup>2</sup>::BK.KA::BL.LA:: $\frac{7}{64}$ : $\frac{5}{36}$

$$\begin{aligned} \text{Chord AH} &= \frac{\text{KM}}{\text{LN}} \text{AG} = \sqrt{\left(\frac{7}{64} \cdot \frac{36}{5} = \frac{7.9}{8.10}\right)} \text{AG} = \text{AG} \sqrt{\frac{63}{80}} \\ &= \text{side of square number } 314159265, \text{ true to the eighth decimal.} \end{aligned}$$

Angle FDE (for  $\frac{13}{50}$ ) =  $14^{\circ} 34' 27''.18$ ; for  $\frac{35}{38}$  =  $42^{\circ} 48' 48''.13$

$$\text{GAB} = 2^{\circ} 57' 40'' \cdot 87.$$

For true chord AH,

$$\text{arc HA} = 124^{\circ} 48' 20'' \cdot 7887; \text{HCB} = 55^{\circ} 11' 39'' \cdot 2112;$$

$$\text{HAB} = 27^{\circ} 35' 49'' \cdot 6556 = \frac{1}{2} \text{HCB}.$$

The omitted  $\cdot(8) 358979 \dots$  in  $\pi$  is  $1 \cdot 875146000$ th part, only  $1 \frac{8}{10}$  inch in 25,000 miles (earth's circuit)  $= 0'' \cdot 00045$  in HCB. Moreover, if to  $\frac{289}{640}$  of  $360^{\circ} = 162^{\circ} 33' 45''$  you add

$$\frac{3}{4} (109^{\circ} 28' 16'' \cdot 39 \dots + 116^{\circ} 33' 54'' \cdot 18 \dots) - \frac{3}{2} (138^{\circ} 11' 22'' \cdot 86 \dots),$$

there results  $124^{\circ} 48' 18'' \cdot 633982$ ,—a polyhedric combination only  $2'' \cdot 15$  in defect.

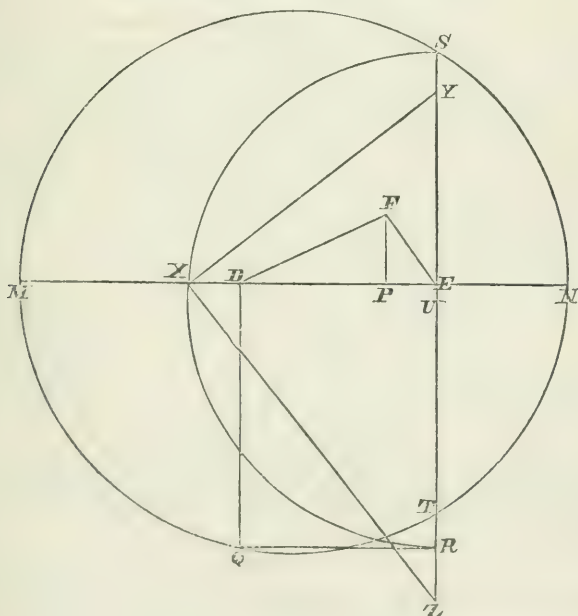
November 30, 1863.

P.S. It is a curious fact that  $3 \cdot 1416 = \cdot 7 \times 1 \cdot 2 \times 1 \cdot 7 \times 2 \cdot 2 =$  product of four numbers in arithmetical progression. Take a  $\Delta$  DFE, right-angled at F,  $DF = 2FE$ , and drop a  $\perp$  FP on base,  $EP = 0 \cdot 2 DE$ . Extend DE to M, N;  $MN = 2 \cdot 9 ED = (2 \cdot 2 + \cdot 7 = \cdot 5 + 1 \cdot 2 + 1 \cdot 2) DE$ , on which as a diameter describe a circle with two opposite ordinates

$$SE = ET = \sqrt{\cdot 7 \times 2 \cdot 2} = \sqrt{1 \cdot 54} DE,$$

and

$$DQ = ER = \sqrt{1 \cdot 2 \times 1 \cdot 7} = \sqrt{2 \cdot 04} DE.$$





On the line  $SR =$  sum of these as a diameter describe a semicircle  $SXR$ , with an ordinate  $EX = \sqrt[4]{1.54 \times 2.04} = \sqrt[4]{3.1416}$  DE. Make  $EY = ED$ ; join  $Y, X$ ; draw  $XZ \perp YX$ , then  $EZ = \sqrt{3.1416}$  DE, up to this limit the side of the square equal in area to the circle whose radius is DE; error is only 1-855275th. The *pentagonal* relations of  $\triangle DEF$  are obvious. Also the Hindu 3927-1250ths. Centre of circle  $MSNTQ$  is at 0.25 DE towards E; that of semicircle  $SXR$  is below E in  $SER$ .

Further,  $\sqrt[3]{\frac{\pi}{6}} = .805995977008235 = 13 \times 31$  into  
 $.00199999001739016 = .\overset{(2)}{2} - .\overset{(7)}{7}1 + .\overset{(10)}{10}175 - .\overset{(13)}{13}11 + .\overset{(16)}{16}16$ ,  
 where  $\overset{(x)}{x}$  denotes  $x$  ciphers after the decimal point. It also  
 equals  $\frac{1}{2} \sqrt{3}$  raised to the  $\frac{3}{2}$  power  $+ .000068528140581$ .

Now  $7(2\sqrt{\frac{1}{3}} - \sqrt{\frac{1}{2}} - \sqrt{\frac{1}{5}}) = .002661131849229 \dots$  and if its  $\sqrt{=}$   $\perp$  from  $G$  on  $BA$ , there would result  $4HA^2 = 3.141595008043 \dots$ , only 3.4 millionths in error.

January 1864.

#### ON ALBERT DURER'S HEPTAGON-CHORD.—SECOND NOTICE.

BY S. M. DRACH, F.R.A.S.

I rejoice to find that my paper in the *Philosophical Magazine* for November last has evoked Sir W. R. Hamilton's account of Röber's approximation. Herr Röber's *Beiträge* and *Pyramiden* tracts are in the British Museum Library—memoirs of 60 and 28 quarto pages with 4 and 1 lithographic plates. This approximation of  $\frac{2\pi}{7} - 0''.012$  is to 1-15363556th part. I regret no

diagram accompanied Sir W. R. Hamilton's paper, but have easily supplemented one from the analysis in page 125.

If, in the annexed diagram,  $ACB$  be an equilateral triangle, with  $CD \perp$  to  $AB$ , and

$$CM = \left( \frac{1}{7} - \frac{1}{9} = \frac{2}{63} \right) CD,$$

let  $DMK = \frac{1}{8} DCA = 3^\circ 45'$ ; then

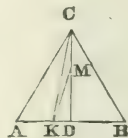
$$CK = .86776704126,$$

which is in defect only .000000437, or 1.2 millionth nearly.

M. Willich finds hept. ch.: rad.: : 105:121 nearly; error 1-777500th:

also  $\frac{105}{121} - \frac{1}{9 \cdot 10^5}$  is wrong by 1-173 millionth, or 0.  $\overset{(8)}{8}5$ .

February 1864.



THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

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[FOURTH SERIES.]

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MAY 1864.

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LIII. *On the Physical Cause of the Glacial Epoch.* By E. FRANKLAND, F.R.S., Professor of Chemistry in the Royal Institution\*.

**A**MONGST the causes that have profoundly influenced the present physical condition of our earth, the action of ancient glaciers upon a scale of almost inconceivable magnitude has gradually but irresistibly forced itself upon the notice of philosophers, since their attention was first called to it by Venetz† in Switzerland, and Professor Esmark‡ in Norway. There are few elevated regions in any quarter of the globe which do not exhibit indubitable evidence of the characteristic grinding and polishing action of ice-masses, although at present perhaps they are scarcely streaked by the winter's snow. The researches of Dr. Buckland and others first revealed the evidence of this ancient glacial action in Great Britain; but it is especially to Professor Ramsay that we are indebted for our present extensive knowledge of the effects produced in this country during the glacial epoch. His explorations have demonstrated that the Highlands of Scotland, and the mountains of Wales and Cumberland, to which I would add the limestone crags of Yorkshire, abound in these *roches moutonnées*, which leave no doubt that the valleys of these mountain-ranges were once filled with glaciers of dimensions unsurpassed, if even equalled, by those which at the present day stream down the sides of their gigantic Swiss rivals. Not only was there this development of perennial ice where no such phenomenon is now observed, but the glaciers of the pre-

\* Communicated by the Author.

† Transactions of the Swiss Natural History Society, vol. i. part 2 (1821).

‡ Edinburgh New Phil. Journ. vol. iii. (1827).

sent age existing in Switzerland, Norway, and elsewhere are evidently but the nearly dried-up streamlets of ancient ice-rivers of enormous size. These glaciers have eroded the alpine valleys of which they once held possession, have scooped out the lochs and kyles of Scotland, as well as the grander fjords of Norway, and have contributed in a most essential manner to the present aspect of our mountain scenery. Ramsay\* and Tyndall have recently called attention to this action of ancient glaciers, and have contended, the former that the lake-basins, the latter that the valleys of the Alps have been scooped out of a comparatively uniform surface.

In no part of the world perhaps can the problem of the glacial epoch be more advantageously studied than in Norway, on whose ice-scarred coasts and fjords two of the essential portions of the glacial apparatus—the ocean and the mountains—are constantly and contemporaneously under the eye of the traveller. 2000 miles of coast, from Christiania to the North Cape, afford almost uninterrupted evidence of the vast ice-operations which, during the epoch in question, moulded nearly every feature of this remarkable country. In this respect Norway has already invited the researches of Esmark, Von Buch, and especially of Professor James Forbes, whose laborious explorations and acute philosophical reasoning have most materially increased our knowledge of the physical phenomena of Scandinavia. To his work on Norway and its glaciers I am indebted for many of the data employed in the following pages. It was likewise during a vacation trip to Norway last summer that I received the impressions regarding the physical cause of the glacial epoch which form the subject of this paper.

Starting from Christiania coastwise, the traveller cannot fail to remark the peculiar appearance of the gneiss and granite rocks composing the coast and the innumerable islands which, forming a natural breakwater, protect the mainland from the heavy seas rolling in from the Atlantic. These rocks, here rarely rising to the height of 800 or 900 feet, present nothing of that sharp and rugged outline which generally characterizes such formations; on the contrary, they are smooth even to their summits, all their angles worn off, and every trace of boldness and asperity effaced. To the casual and uninstructed observer the action of the sea suggests itself as a sufficient cause for these appearances: but it does not require much scrutiny to be convinced that the ocean waves have had little to do with this smoothing and polishing of the coast. The want of uniformity in structure and chemical composition of masses of gneiss and granite causes them to be unequally acted upon by water. The latter dissolves and dis-

\* Quart. Journ. Geol. Soc. August 1862.

tegrates the more soluble and friable parts, producing a cellular or fissured surface according to circumstances, instead of the smoothed and comparatively polished condition which obtains on this coast. Such a weathering of the rocks can in fact be plainly seen in many places superposed upon the polishing due to the more remote agency of moving masses of ice. In short, it may be stated as a general proposition, that the action of the sea and weather upon rock produces a corrugated, irregular, and especially a fissured surface; whilst that of glacier-ice occasions a rounded, comparatively smooth and uniform appearance. If further proof of the exclusively glacial origin of the surface of the Norwegian coast-rocks be needed, it may be found, first, in the frequent occurrence of the scratchings and fine markings which so incontestably characterize rocky surfaces that have formed the bed of a glacier, and, secondly, in the circumstance that the smoothing action has evidently come as a rule from the direction of the land. All the surfaces inclined towards the land are invariably, as far as I have seen, rounded and polished, whilst in some places, where the rock descends precipitously towards the sea, it has been protected from the abrading action and presents merely a weathered surface.

Doubling the Naze and proceeding northward, the coast presents, with slight exceptions, the same general features until the arctic circle is approached, when the character of the scenery rather suddenly changes. The rocky hills acquire the dignity of mountains, and tower up in rugged, sharp, and fantastic peaks, contrasting strongly with the rounded summits of the lower latitudes. But these arctic peaks owe their immunity from the abrading action of ice solely to their height; around their bases, and even high up their sides, the slow surges of the moving sea of ice have made their unmistakeable marks, having ground and even undercut them into most extraordinary forms,—as instances of which I may mention the Seven Sisters, and Torghatten with its singular tunnel, south of the arctic circle, the Hestmand standing upon the circle, and the mountains of the Folden and Vest fjords, the latter having been graphically compared by Mr. Everest to the jaws of an immense shark.

It is important to determine the direction of the striæ upon these *roches moutonnées*. So far as my own observations go, they confirm the statement of M. Siljeström, that the abrading action proceeded generally in a N.W. direction; that is, from the land towards the sea. In cases where there was a variation from this general direction, some sufficient cause, such as the embouchure of a fjord, or the interposition of an obstacle seaward, presented itself. I could not, therefore, resist the conclusion that the masses of ice which caused these striæ moved down the mountain-slopes



to the sea, and did not visit the coast as floating masses from the polar regions. I must not omit to state, however, that on this point Forbes inclines to a different opinion. He says\*, "I cannot pronounce on the direction of the striæ, which I could not land to examine. It rather appeared to me, however, that on the coast at least the direction of friction, marked by the *stos* and *lee Seite*, was *parallel* to the coast and from north to south."

However this may be with regard to the coast, the exploration of several of the fjords convinced me that the ancient glaciers followed tracks leading them from the gathering-basins of the mountains by the nearest available route to the sea. The Hardanger, Romsdal, Trondliem, Namsen, and Salten fjords exhibit everywhere the most unmistakeable evidence that they were once filled with vast glaciers, to which, in fact, those fjords without doubt mainly owe their existence. The Hardanger, with its modern glaciers which stream down from the *névé* of the Folge Fond, is a magnificent example of the channel of an ancient ice-river. Wherever its rocky shores are bare, they are scored with the characteristic flutings; the position of the latter, and the freedom from abrasion of those surfaces which are precipitously inclined towards the mouth of the fjord, plainly proclaim the direction in which this gigantic glacier moved. In the Romsdal, at the head of the fjord of that name, the evidence of former glacial action is perhaps still more striking, because it concentrates itself more closely around the traveller. As far as the eye can define, up the precipitous walls of this grand ravine, the rocks are grooved and planed down by the action of moving ice; and immense gneiss blocks which now lie at the bottom of the gorge, but whose former position can be traced to near the summit of the precipices, show by the smoothed and streaked character of their formerly exposed sides, either that this gorge was once completely filled with ice, or that it was gradually scooped out by a glacier of more moderate thickness.

It was natural that these accumulating evidences of a former condition of the surface of our planet, so different from that which now obtains, should call forth various hypotheses intended to account for a thermal state which permitted the occupation, by such vast masses of ice, of tracts of land which now frequently yield rich pasturage and luxuriant crops. It was suggested by Fourier that the temperature of space is not uniform; and that our solar system, in performing the proper motion among the stars which is believed to belong to it, sometimes passes through regions much colder than others. According to this hypothesis, the glacial epoch occurred during the passage of our system through a comparatively cold portion of space. Some

\* Norway and its Glaciers, p. 46.

have imagined that the heat emitted by the sun is subject to variation, and that this epoch was the result of what may be termed a cold solar period. Mr. W. Hopkins believes that a different distribution of land and water, and especially a different direction of the currents of warm water which set from the tropical towards the polar oceans, would render the climate of certain localities colder than it is at present, and would thus sufficiently account for the phenomena of the glacial epoch. Finally, Professor Kämtz\* considers that at the time of the glacial period the mountains were much higher than at present, Mont Blanc reaching an altitude of 20,000 feet for instance, the secondary and tertiary formations having been removed from their summits during the glacial epoch.

The two last assumptions are attended with formidable geological difficulties, especially when it is considered that the phenomena of the epoch in question extended over the entire surface of the globe; they have therefore never acquired more than a very partial acceptance†. The first two hypotheses, again, have been recently shown by Tyndall to be founded upon an entirely erroneous conception of the conditions necessary to the phenomena sought to be explained. The formation of glaciers is a true process of distillation, requiring heat as well as cold for its due performance. The produce of a still would be diminished, not increased, by an absolute reduction of temperature. A greater differentiation of temperature is required to stimulate the operation into greater activity. Professor Tyndall does not suggest any cause for such exalted differentiation during the glacial epoch; but he proves conclusively that both hypotheses, besides being totally unsupported by cosmical facts, are not only incompetent to constitute such a cause, but also assume a condition of things which would cut off the glaciers at their source, by diminishing the evaporation upon which their existence essentially depends. Only by a greater difference of temperature between land and ocean is an increase of glacial action possible; and these hypotheses fail, inasmuch as they ignore altogether the necessity for such an augmentation of thermal difference.

\* *Mittheilungen der k.-k. geographischen Gesellschaft zu Dorpat*, vol. ii.

† Speaking of the glacial epoch, Ramsay says (*Quart. Journ. Geol. Soc.* for 1862, p. 204), "I find it difficult to believe that the change of climate that put an end to this could be brought about by mere changes of physical geography. The change is too large and too universal, having extended alike over the lowlands of the northern and the southern hemispheres. The shrunk or vanished ice of mountain-ranges is indeed equally characteristic of the Himalaya, the Lebanon, the Alps, the Scandinavian chain, the great chains of North and South America, and of other minor ranges and clusters of mountains like those of Britain and Ireland, the Black Forest, and the Vosges."

This will be apparent from a consideration of the functions of the three essential parts of the great natural glacial apparatus, viz. the evaporator, the condenser, and the receiver. The part performed by the ocean as the evaporator is too obvious to need description. The two remaining portions of the apparatus, however, are generally confounded with each other. The mountains are in reality the receivers or *ice-bearers*, and are only in a subordinate sense condensers. The true condenser is the dry air of the upper region of the atmosphere, which permits of the free radiation into space of the heat from aqueous vapour\*, the latter, as proved by Tyndall's recent researches, possessing extraordinary powers of radiation and absorption. He has shown that the watery vapour at the upper surface of a stratum of air saturated, or nearly so, with moisture, must rapidly radiate its heat into space and condense to rain or snow, according to the temperature of the surrounding atmosphere, dry air being almost completely powerless to arrest this radiation. Thus it is that the enormous amount of heat developed by the condensation of aqueous vapour is got rid of without any appreciable elevation of the temperature of the medium in which the operation occurs. That this process of condensation must be most active and important in meteorological phenomena can scarcely be doubted, when it is considered that the great accession of heat to the surrounding atmosphere, which occurs when aqueous condensation takes place from any other cause, must soon put a stop to the further deposition of moisture under such circumstances. Thus the condensation of one cubic foot of water at  $40^{\circ}$  from aqueous vapour at  $32^{\circ}$  F. would raise the temperature of 352,053 cubic feet of air through  $10^{\circ}$ . Such an enormous accession of heat, where condensation takes place without radiation, could not fail promptly to arrest the process.

Thus the condenser is an apparatus perfectly distinct from the

\* I have devised a simple mode of experimentally demonstrating the radiation from aqueous vapour, so that the effect can be seen by a large number of persons at once. A charcoal chauffer, 14 inches high and 6 inches in diameter, is placed about two feet from, and in front of, a thermoelectric pile, the radiation from the chauffer and fuel being carefully cut off from the pile by a double metallic screen. The deflection of the galvanometer due to the radiation from the ascending and heated carbonic acid being now carefully neutralized by a constant source of heat radiating upon the opposite face of the pile, a current of steam is made to ascend through an iron tube passing vertically through the chauffer. Instantly the galvanometer deflects for heat much more powerfully than it did previously to its compensation, when it was exposed to the full radiation from heated air and carbonic acid. When the current of steam is interrupted, the needle immediately returns to zero. If now a current of air be forced up the central tube instead of steam, either no deflection at all, or a slight one for cold occurs. The heat of the chauffer effectually prevents any condensation of the steam.



ice-bearer, which last occupies in fact the position of the receiver in ordinary distillation ; and hence so long as the temperature of the ice-bearers, inclusive of the surrounding atmosphere, does not rise above  $0^{\circ}\text{C.}$ , their functions remain intact. Other things equal, an ice-bearer at  $0^{\circ}\text{C.}$  is scarcely surpassed in efficiency by one at  $-15^{\circ}\text{C.}$  But it must be borne in mind that the actual efficiency of an ice-bearer, on an annual average, is dependent upon the length of time during which its temperature does not rise above the freezing-point. Hence it is that those mountains which penetrate furthest into the increasingly cold heights of the atmosphere are most efficient,—not because their temperature is sometimes far below the freezing-point, but because they maintain a temperature below that point throughout a greater portion of the year.

These considerations lead to the conclusion that, assuming the supply of aqueous vapour to the atmosphere to remain constant, increased condensation could only arise from greater facilities for the radiation from that aqueous vapour into space,—a condition which involves cosmical changes of which we have not the slightest evidence. On the other hand, as the powers of the radiating condenser are at the present moment far from being taxed to the utmost, the more copious supply of aqueous vapour to the atmosphere would at once produce a corresponding increase of condensation. Such an increased supply of aqueous vapour, extending over a considerable period of time, could only arise from the association of a greater amount of heat with the waters of the ocean. But all the hypotheses hitherto propounded to account for the glacial epoch have failed to recognize this feature of the problem, inasmuch as they have all assumed that cold alone was necessary to the development of the phenomena of that epoch. This important omission must serve as my apology for advancing a new hypothesis, which necessarily rests in some respects upon data as yet imperfectly ascertained, and which has gradually elaborated itself out of the impressions I received during my recent visit to Norway. Any such theory must take cognizance of the following points in the history of the glacial epoch. 1st. That its effects were felt over the entire globe. 2nd. That it occurred, or at least terminated at a geologically recent period. 3rd. That it was preceded by a period of indefinite duration, in which glacial action was either altogether wanting, or was at least confined to regions of considerable altitude. 4th. That during its continuance, atmospheric precipitation was much greater, and at one period the height of the snow-line considerably less than at present. 5th. That it was followed by a period extending to the present time, when glacial action became again insignificant.



All these conditions would be the natural sequences of a gradual cooling of the ocean from a higher down to its present temperature. *The sole cause of the phenomena of the glacial epoch was a higher temperature of the ocean than that which obtains at present.*

This hypothesis rests chiefly upon the two following propositions:—

1st. That a higher oceanic temperature would give rise to an increased evaporation, and consequently to an augmented atmospheric precipitation.

2nd. That this increased atmospheric precipitation would augment the average depth of permanent snow upon the ice-bearers, and would, within certain limits, depress the snow-line.

Let us now examine these propositions in detail. As a general proposition, the first is an indisputable truth, and it is therefore only necessary to inquire into the extent to which oceanic evaporation would be thus affected. The rate of evaporation of water at different temperatures and under various circumstances was determined by Dalton, whose results are embodied in the following Table. The evaporation took place in each case from a circular surface 6 inches in diameter:—

Temp. F.	Evaporation per minute in calm.	Evaporation per minute in breeze.	Evaporation per minute in high wind.
	grs.	grs.	grs.
85°	4.92	6.49	8.04
75	3.65	4.68	5.72
65	2.62	3.37	4.12
55	1.90	2.43	2.98
45	1.36	1.75	2.13
35	.95	1.22	1.49

We have no sufficient data for calculating the present mean temperature of the ocean; but in lat. 69° 40', off the coast of Norway, at noon on a remarkably hot summer's day, Forbes found the surface temperature to be 46°·5 F. The assumption of 40° F. as the mean temperature off the coast of Norway will therefore probably be in excess of the truth. Now, taking the mean of Dalton's results obtained at 35° and 45°, the evaporation in each minute from a circular surface 6 inches in diameter would be at 40°:—

In calm.	In breeze.	In high wind.	Mean.
1.15 grain.	1.48 grain.	1.81 grain.	1.48 grain.

And further, the evaporation from a similar surface at 60°, according to the mean of the results obtained by Dalton at 55° and 65°, would be as follows:—

In calm.	In breeze.	In high wind.	Mean.
2·26 grains.	2·90 grains.	3·55 grains.	2·90 grains.

These absolute numbers were obtained with dry air, and therefore they cannot be taken as representing the *actual* evaporation from a surface of water like that of the ocean, which is in contact with air of ever-varying hygrometric qualities. All I contend for is, that Dalton's numbers represent, under similar conditions in the two cases, the *relative*, but not the absolute evaporation from a given oceanic surface; and this being granted, it follows that an elevation of 20° F. above the present temperature of the ocean bathing the shores of Norway would double the evaporation from a given surface. Such an increased evaporation, accompanied as it necessarily must be by a corresponding precipitation, would suffice to fill the fjords and cover the western coast of that country with ice, provided that the ice-bearers were in a sufficiently effective condition. But would not the increased oceanic temperature tend to augment the mean temperature of the atmosphere even at considerable elevations, and thus to raise the snow-line and reduce the area of perpetual snow?

The reply to this question is contained in the second of the above propositions. That the limit of perpetual snow does not entirely depend upon the mean temperature of the atmosphere at that particular elevation is conclusively proved by the very different mean temperature of the snow-line in various localities. Thus, under the equator it is about 35°; in the Alps and Pyrenees 25°; and, according to Von Buch, in lat. 68° in Norway, it is only 21°\*. These numbers are very instructive; why does the mean temperature of the snow-line rise as we approach the equator? The answer to this question has been already given by Mr. Hopkins in his admirable memoir on the Influence of the Earth's Secular Heat upon Climate†. He considers that the low snow-line in the tropics is due to a more equable temperature and greater atmospheric humidity. The deluges of rain which fall within the tropics far surpass the precipitation in the temperate and frigid zones; and doubtless the fall of snow upon most inter-tropical mountains is proportionately great. The important influence which the amount of precipitation alone exercises upon the lower limit of perpetual snow is strikingly exemplified at the fine waterfall of Tyse Strenger near the head of the Hardanger Fjord, and was first noticed by Mr. M. Williams‡. The spray from this fall, being frozen in winter, covers the valley for nearly half a mile with a stratum of snow and ice so thick as to defy the solar

\* Humboldt's *Cosmos*, vol. i. p. 9. Forbes's 'Norway,' p. 205.

† Geological Society's Journal, vol. viii. p. 78.

‡ Through Norway with a Knapsack.

rays of summer to melt it. I have also seen in the Sör Fjord, under similar abnormal conditions, a mass of snow lying early in August last, within 10 feet of the level of the sea, although the normal snow-line is there at least 4500 feet above the sea-level. Mr. Hopkins calculates that the snow-line near the equator is 1000 feet lower than the mean line of  $32^{\circ}$ , whilst in the Alps and at the Arctic Circle it is respectively 2000 and 3500 feet higher than the line of  $32^{\circ}$ . Hence by the influences above mentioned, the snow-line at the equator is brought down no less than 4500 feet below its thermal position at the Arctic Circle; and therefore, with the equatorial precipitation, and a more equable temperature than at present, the snow-line in Norway would descend to the sea at the Arctic Circle from its present position of between 3000 and 4000 feet above the ocean. This essential dependence of the height of the snow-line upon the amount of precipitation and equable temperature is also confirmed by its comparative height on the coast and in the interior of the Scandinavian peninsula, as given by Forbes in the following Table, compiled partly from his own observations, and partly from those of Von Buch, Naumann, and others\* :—

Latitude.	Height of snow-line in feet.		
	Interior.	Coast.	Difference.
60	5500	4450	1050
62	5200	4150	1050
64	4200	3650	550
66	3700	3250	450
68	3450	3000	450
70	3350	2900	450

Thus the difference between the height of the snow-line near the coast, where, owing to the impact of the Gulf-stream, the winter is mild but the atmospheric precipitation great, and in the interior, where the climate is one of extremes, and the air is comparatively dry, amounts in some cases to as much as 1050 feet, or nearly one-fourth of the total height. Nor must it be forgotten, in connexion with this depression of the snow-line, that copious precipitation is altogether incompatible with great summer heat. The incessantly clouded sky cuts off the solar rays and moderates the summer temperature. It is a trite observation, that a wet summer is always a cold one.

Whilst, therefore, the tendency of augmented oceanic warmth would be to raise the mean temperature of the limit of perpetual snow, there would nevertheless be, within certain limits, a

\* Norway and its Glaciers, p. 214.



depression of the snow-line itself from the attendant circumstances just enumerated: viz., 1st, from increased precipitation, which would cause such an accumulation of snow during the winter months, as would defy the heat of the succeeding summer to melt; for it must be borne in mind that the amount melted is proportional, not to the mass, but to the surface exposed to the thawing influence; and 2nd, from the reduction of summer temperature, owing to the interception of the sun's rays by an overcast sky. But it is evident that this lowering of the snow-line by increased oceanic temperature could only occur within certain limits; for although the mean temperature of the snow-line might rise from  $21^{\circ}$  (its present position in Norway) to  $35^{\circ}$ , its height under the equator, and perhaps even still higher, without any elevation of the snow-line itself, yet a further rise of mean temperature, which would result from a continued augmentation of oceanic heat, could not fail to elevate the snow-line itself, and eventually to chase the last portions of snow even from the loftiest mountain peaks. A process the inverse of this I believe to have gone on in nature, leading gradually to the so-called glacial epoch, and eventually to the present meteorological condition of our globe. The ocean once possessed a temperature so high that the snow-line floated above the summits, possibly even of the most lofty mountains; but with the reduction of oceanic warmth it gradually descended, enveloping peak after peak in a perennial mantle, until during the most severe portion of the glacial epoch it attained its lowest depression, whence it again rose to its present position, owing to diminished evaporation, the effect of which, in elevating the line of perpetual snow, has already been explained.

Having thus endeavoured to prove that all the phenomena of the glacial epoch would be normally evolved by the gradual cooling of the ocean from a higher degree of heat down to its present temperature, it remains for me to suggest a cause of such a higher oceanic temperature, and to remove certain geological and palæontological objections which may be urged against this hypothesis.

First, as regards the cause of the assumed higher temperature, it may be stated as an exhaustive proposition that the accession of heat must either have been from without, from a cosmical source, or from within, that is, of secular origin. Now only two cosmical causes of the variation of terrestrial temperature have yet been suggested; and to these I have already alluded. The first of them, viz. the hypothesis which assumes that our solar system may have passed through portions of space in which it would receive more heat-radiations from the stars, has been elaborately reviewed by Mr. Hopkins in the memoir



already quoted, and he has conclusively shown it to be quite untenable on astronomical grounds. The second requires some consideration, since it is by no means improbable that the sun was once a more active radiator of heat than he is at present, although the assumption that this should have been the case to a marked extent so recently as the close of the glacial epoch is in the highest degree improbable. It might be anticipated that an augmentation of solar energy would increase the temperature of the oceanic surface, and probably not interfere much with the functions of the radiating condenser, the luminous heat-rays of the sun not having yet been proved to be much absorbed by aqueous vapour. But it would doubtless greatly impair the efficiency of the ice-bearers, whose powerfully absorbent surfaces would, under increased solar radiation, be less able to maintain the necessary low temperature. Fortunately, however, it is not necessary to rely upon conjecture in this matter, for the demonstration is constantly before us, in the comparative effects of solar radiation upon glacial phenomena in the different terrestrial zones. The solar radiation within the tropics is augmented as compared with that of the temperate and frigid zones, but this augmentation does not bring on a glacial epoch in the torrid zone; on the contrary, although the snow-line is, as above stated, depressed as regards the line of  $32^{\circ}$ , yet both are irresistibly driven up the mountains.

Thus no cosmical source of heat is competent to produce the phenomena of the glacial epoch, and there remains therefore only the well-known secular source—the internal heat of the earth. The problem of the influence of the earth's internal heat upon the surface temperature has been most ably discussed by Mr. Hopkins\* and Professor W. Thomson†. Taking the result of Poisson's calculation, that the part of the earth's present superficial temperature due to primitive heat amounts to only one-twentieth of a degree Fahrenheit, Professor Hopkins proves that the augmentation of surface temperature from this cause to the extent of  $10^{\circ}$  F. would involve a descending rate of increase so rapid as to reach  $200^{\circ}$  F. at a depth of only 60 feet,—a physical condition of our planet which would, as he remarks, scarcely be consistent with the conditions of animal life at the more recent geological epochs. Assuming Poisson's datum to be correct, this result is probably indisputable, as applied to the solid surface of our planet; but it is not necessarily so when extended to the floor of the ocean. Indeed there are some considerations regarding the transmission of heat through a thick stratum of water, which appear to me to render this calculation totally in-

\* Journ. of Geol. Soc. vol. viii. p. 56; and Phil. Trans. 1857, p. 805.

† Transactions of the Royal Society of Edinburgh, vol. xxiii. p. 157.

applicable to the determination of the past influence of internal heat upon the surface temperature of the ocean. There are four circumstances which require consideration in attempting to realize the thermal condition of the ocean during the cooling of the earth's crust. These are, first, the conductivity of water; secondly, its convection; thirdly, its power of penetrating into any chasms or dislocations that might be formed from time to time in the floor of the ocean; and fourthly, its specific heat.

With regard to the conductivity of water, the determinations of Despretz\* show that, compared with the conductivity of the solid crust of the earth, it may be regarded as a vanishing quantity. But this almost total absence of conducting-power is more than compensated for by fluid convection. If the ocean were free from lateral currents, however, even this convection would not be much superior to the conductivity of granite in transmitting heat to the surface; for if we assume the average depth of the sea to be five miles, then, even if its temperature at the bottom were  $100^{\circ}$  C., and that at the surface  $15^{\circ}$  C., we should only have a difference of  $.0032^{\circ}$  C. for each foot of ascent. There are no determinations of the velocity of convection in seawater for given differences of temperature, but, for the small foot-difference contemplated, it must be almost inconceivably small. Nevertheless it can scarcely be doubted that the polar and equatorial currents aid this vertical convection to such an extent as to render the total transfer of heat from the floor to the surface of the ocean considerably more rapid than that which would take place through a solid stratum of granite of the same thickness. I have endeavoured roughly to determine the relative powers of water and granite in thus transmitting heat. For this purpose a 6-inch cube was cut out of a block of granite, and a vessel of tin plate was prepared capable of holding a similar cube of water covered with a layer of oil  $\frac{1}{6}$ th of an inch thick to prevent evaporation. Both cubes were placed upon an iron plate heated below by an atmosphere of steam, their vertical sides being protected from loss of heat by several folds of flannel. Into each of their upper surfaces, which were exposed to the air, the bulb of a thermometer was imbedded, and the time required to elevate the surface temperature  $10^{\circ}$  C. was noted. The following are the results of these experiments:—

Time required to raise temperature of surface of granite cube through $10^{\circ}$ C. . . .	} = 1 <sup>h</sup> 10 <sup>m</sup>
Time required to raise temperature of surface of water cube through $10^{\circ}$ C. . . .	
	} = 57

\* *Ann. de Chim. et de Phys.* vol. lxxi. p. 206.

It is necessary to mention that the surface of the tin-plate cube in contact with the hot plate was coated with a film of lampblack, and that the surfaces of the granite were only roughly ground and not polished; in fact all the experimental conditions were in favour of the transmission through water, and yet the surface temperature of the latter did not rise with much greater rapidity than that of the former. Now taking into account the specific heats and gravities of water and granite, the amount of heat transferred from the base to the summit of each block will be in the following proportion for equal times:—

Granite : Water.

1 : 2·36.

Although these determinations can only give a rough approximation to the respective velocities with which heat passes through strata of granite and water, yet they show that the convection of heat through the latter is by no means so rapid, when compared with its conduction through granite, as is commonly supposed. There can be no doubt that, in the case of the ocean, the rapidity of transfer would be increased by polar and equatorial currents; nevertheless the assumption that the cooling of the floor of the ocean proceeded as rapidly as if it had been freely exposed to the air is altogether untenable; and I conceive it not only possible but probable that this secular cooling of the earth through the ocean may have continued down to a comparatively very recent geological period, and may, even at the present day, not be altogether interrupted.

The greater facility with which heat is thus conveyed through water would obviously render the rate of downward increase of warmth for a given surface temperature much less rapid than in the case of granite. Thus I consider it probable that the internal heat of the earth affected in a very marked degree the surface temperature of the ocean long after it had ceased to influence appreciably the external warmth of the land. This assumption acquires considerable support from a consideration of the conditions tending to retard the escape of heat from the oceanic surface as compared with that from the land. The facility with which radiant heat escapes from equal surfaces of water and granite at the same temperature through perfectly dry air is nearly equal; but so soon as aqueous vapour is interposed in the path of these rays, the conditions become wonderfully altered; the escape of heat from both is diminished, but its radiation from the water is retarded in by far the greatest degree. This extraordinary intranscendency of aqueous vapour to rays issuing from water has been conclusively proved by Tyndall in a paper just communicated to the Royal Society\*.

\* Proceedings of the Royal Society, vol. xiii. p. 160.



Whether we take into consideration, therefore, the diffusion of heat through water and granite, the quality possessed by the ocean of abstracting heat, through the agency of dislocations, &c., from depths still more profound than its own floor, or finally the respective facilities with which, under the cosmical conditions contemplated, water and granite throw off their heat into space, we find everywhere a state of things tending not only to the greater conservation of the heat of the water, but also to a less rapid increase of temperature from the surface downwards, than is the case with the solid crust of the globe; and this applies also, *mutatis mutandis*, to the retention of that heat which is received from solar radiation. The luminous heat-rays of the sun pass freely through aqueous vapour, and are absorbed by both granitic and oceanic surfaces; but once absorbed, these rays issue forth again as obscure heat of two different qualities, or rates of vibration. To use Tyndall's explanation of the phenomenon, the vibrations of the liquid water-molecules are of such rapidity as can be best taken up and absorbed by the same molecules in the vaporous condition. But granite is a very complex substance, and fewer of the heat-oscillations of its atoms are in unison with those of aqueous vapour; hence the heat-vibrations of granite disturb the molecules of aqueous vapour in their passage through the atmosphere in a less degree, and consequently the granite rays are less absorbed.

Thus the chief process by which the ocean lost heat was evaporation; for this, as is well known, proceeds at a surface of water until the superambient air is saturated with vapour. The latter, by virtue of diffusion, rises into the presence of the dry-air condenser, there to yield up its latent heat and be converted into rain or snow, according to the temperature of the medium in which the radiating process takes place. Hence the one heat-dissipating process of which the water is most capable, is that by which the ice-bearers are provided with their snowy burthen.

*Objections.*—To the hypothesis which I have endeavoured to develop in the preceding pages, I am aware that, in addition to those already alluded to, several objections may be urged which appear at first sight very formidable, but which, I think, lose this character, to a great extent at least, when more closely investigated.

1. Perhaps the most palpable of these objections is one which may be taken to my statement that a more copious atmospheric precipitation would cause a greater accumulation of snow upon elevated portions of land, and thus depress the snow-line. It may be urged that although such an increased atmospheric precipitation would have the effect of depositing more snow in



winter, yet the corresponding heavy fall of rain in summer would again liquefy the excess of snow so deposited. In reply to this objection, it might perhaps be sufficient to oppose the facts above given, regarding the comparative height of the snow-line in adjacent moist and dry localities; but I may also add in explanation of these facts, that a comparatively very large quantity of even warm water is required to melt snow or ice. In fact, as is well known, the amount of heat requisite merely to melt them would, if no melting occurred, raise their temperature to  $174^{\circ}$  F. Let us suppose that upon any given ice-bearer the precipitation throughout the entire year was doubled, that during six months of the year this increased precipitation occurred in the form of snow at  $32^{\circ}$  F., and that during the remaining six months it fell in the form of rain at  $50^{\circ}$  F.; still, even with these conditions, so manifestly unfair towards the ice-bearer, very little more than one-eighth of the additional snow would be melted by the warm rain. In fact it requires nearly eight tons of water at  $50^{\circ}$  F. to melt one ton of snow or ice, even when the latter is already in a thawing condition. Forbes considers that not more than one-fiftieth of the snow upon the snow-fields of Norway is liquefied by the rains of summer; whilst M. Durocher has calculated, from observations made at the convent of St. Bernard in Switzerland, which is slightly *below* the snow-line, that not more than one-ninetieth of the annual snow is dissolved by the rain. Thus the effect of summer rain in melting the snows of winter is comparatively insignificant.

2. Is it not a necessary consequence of this hypothesis that the ocean must have possessed a temperature incompatible with animal life at the comparatively remote protozoic period? This question doubtless raises the most formidable of the objections that can be brought against my view; nevertheless there are several considerations which deprive it of much of its force. Judging from purely geological evidence, the period which has elapsed since marine life first made its appearance, compared with that intervening between the glacial epoch and the present time, would probably not be under-estimated at 1000:1; consequently it would appear to be evident that if the ocean has cooled through, say  $20^{\circ}$  F. during a unit of time, it must have been at a boiling temperature at a far less distant period than 1000 such units. There are three circumstances, however, which forbid such an unqualified deduction. In the first place, the excessive evaporation of water at temperatures not far removed from the boiling-point would rapidly reduce the ocean and its floor to a comparatively moderate temperature. Secondly, the excessive precipitation which must have occurred in the preglacial periods must have accelerated the deposition of most of the sedimentary rocks

to such an extent as to render the proportion above given between the pre- and post-glacial periods considerably too great. Thirdly, the advent of the severer portion of the glacial epoch would necessarily effect a rapid refrigeration of the ocean, and thus magnify the cooling effected during the post-glacial unit of time.

The experiments of Dalton on the rate of evaporation at different temperatures serve to illustrate the truth of the first of these considerations. He found, and his results were afterwards confirmed by Daniell, that the rate of evaporation increases in a geometrical progression with equal increments of sensible heat. From the same cause, the deposition of most of the pre-glacial sedimentary rocks must have also proceeded at a much more rapid rate than that which has obtained since the glacial epoch; in fact the rate of deposition depends so essentially upon the amount of precipitation, as to render it by no means improbable that the rate of decrease from a certain point down to the glacial epoch was also approximately a geometrical one: thus the proportion between the pre- and post-glacial zoic periods would be considerably reduced. Finally, the descent of the snow-line to the level of the sea along an immense length of coast during the glacial epoch must have had the effect of very rapidly reducing the temperature of the ocean, since the precipitation upon the land, instead of reaching the sea after being warmed at the expense of solar heat, would now be thrown into the ocean, partly as ice-cold water, but chiefly as ice itself; and every ton of the latter in melting would cool more than fourteen tons of sea-water through  $10^{\circ}$  F. It must also be borne in mind that this ice-cold water would not float on the surface, as in a freshwater lake, but would at once sink into the subjacent warm water, because the point of maximum density of sea-water is, according to Despretz\*,  $6^{\circ}6$  F. below the freezing-point of pure water. Thus, taking all these circumstances into consideration, and assuming that the ocean has lost as much as  $20^{\circ}$  F. of heat since the glacial epoch, I conceive it possible that there would still be a sufficient interval, between a temperature incompatible with marine animal life and that epoch, for the development of the various organisms inhabiting the pre-glacial seas. In the case of land animals and plants no such difficulty arises, since, as already explained, the surface temperature was, during the whole of the period contemplated, very little affected by the internal heat of the earth.

3. Recent research has led some geologists to the conclusion that glacial action existed in the Miocene and even so far back as the Permian period; and although the evidence upon which this conclusion rests is by no means generally accepted by geo-

\* *Ann. de Chim. et de Phys.* vol. lxx. p. 45.

logists, still I may as well here remark that such comparatively remote glacial action is perfectly compatible with the views I am here advocating. It is in fact a necessary consequence of these views, that the so-called glacial epoch should not be a sharply limited period, although its termination, for the reasons just given, would probably be much more definite than its commencement. I have already argued that perpetual snow would first tip the mountain peaks, and then slowly and gradually descend to the sea-level. But it must be borne in mind that during the whole of the pre-glacial period the atmospheric precipitation was even greater than during that period, and consequently wherever the land rose well above the snow-line, glaciers, on a scale far surpassing any of the present time, would be the inevitable consequence. It is, I believe, exceedingly difficult for a geologist to reconstruct even an approximation to the contour lines of land during the Permian period. Nevertheless in suggesting a glacial episode during this period, Ramsay\* believes that he has considerable evidence of the existence of a range of hills whence these glaciers descended.

Such considerations respecting the pre-glacial period present to us a vivid picture of the then terrestrial climate. The shores of the warm seas would possess a genial and remarkably equable temperature, the air always warm and moist, the earth screened from the summer's sun by a clouded sky, and protected from the cold of winter by a canopy of transparent aqueous vapour, impervious to terrestrial radiation. Receding from the coast on level ground, these peculiarities would become gradually fainter; but on a somewhat steep shore the approximation of warm and cold climates must have been much closer than at present. The rivers and lakes fed by mountain-snows would also present a marked contrast to the surrounding lowlands—thus exhibiting within a narrow region a wide range of temperature, adapted to the thermal habits of widely different organisms.

As the glacial epoch approached, this genial zone would be gradually narrowed by the progressive descent of the snow-line; for although the actual amount of heat in activity at the surface of the earth was greater during the glacial period than subsequently, yet the cold of winter became stored up in masses of falling snow, which in melting absorbed the heat of the succeeding summer, and thus reduced both the mean and summer temperature of such tracts of land as were not situated greatly below the snow-line. Nor was this downward march of a frigid climate arrested even by the ocean itself, but the vast flow of glacial ice into the sea formed a belt of cold water along the shores, where many marine organisms that have left their traces

\* Proceedings of the Geological Society, August 1855.



in the glacial drift found a congenial temperature. The common notion, therefore, that the glacial epoch was a cold period is correct, although heat, and not cold, was the *cause* of that epoch. This apparent paradox, that heat should be the cause of cold, finds its parallel in the ice-making machines which were in operation at the last Great Exhibition. In those machines, which produced from 2 to 12 tons of ice per ton of coal, the glacial produce was directly proportional to the amount of heat developed by the combustion of coal.

Such are the principal objections to my hypothesis which have occurred to myself or been suggested by others; and although I have thus endeavoured to remove them, yet it would be idle to deny that some of them retain considerable force, or that others, perhaps even more formidable, may occur to the minds of those who possess a far more extensive knowledge of the phenomena of the glacial epoch than I can pretend to. No hypothesis can be considered as of any value until it has been thoroughly sifted, and by the result of this necessary process the one I have now ventured to suggest must either stand or fall. Meanwhile it commends itself by requiring the assumption of no natural convulsion or catastrophe, no vast or sudden upheavals or depressions, and no change in the thermal relations of our earth to the sun or to space. On the contrary, it insists that the glacial epoch was normally and gradually evolved from a thermal condition of the interior of our globe which can scarcely be said to be any longer the subject of controversy.

In conclusion, this hypothesis suggests the probability that the other bodies belonging to our solar system have either already passed through a similar epoch or are destined still to encounter it. With the exception of the polar ice of Mars and the bright clouds of Jupiter, we have hitherto obtained no certain glimpse into the thermal or meteorological condition of the planets: neither is the physical state of their surfaces accessible to our best telescopes. It is otherwise, however, with the moon, whose distance is not too great to prevent the visibility of comparatively minute details. A careful observation of the lunar surface for more than a year with a silvered-glass reflector of 7 inches' aperture and of good defining power, has created in my mind an impression that our satellite has, like its primary, also passed through a glacial epoch, and that several at least of the *valleys*, *rills*, and *streaks* of the lunar surface are not improbably due to former glacial action. Notwithstanding the excellent definition of modern telescopes, it cannot be expected that other than the most gigantic of the characteristic details of an ancient glacier-bed should be rendered visible. Under favourable circumstances the terminal moraine of a glacier attains to enormous dimensions;



and consequently, of all the marks of a glacial valley, this would be the one most likely to be first perceived. Two such terminal moraines, one of them a double one, appear to me to be traceable upon the moon's surface. The first is situated near the termination of that remarkable streak which commences near the base of Tycho, and passing under the south-eastern wall of Bullialdus, into the ring of which it appears to cut, is gradually lost after passing crater 216 (Lubinietzky). Exactly opposite the last crater, and extending nearly across the streak in question, are two ridges curved towards the north, and reminding the observer of the concentric moraines of the Rhone Glacier. Beyond the second and outermost ridge a talus slopes gradually down northwards to the general level of the lunar surface. These ridges are visible for the whole period during which that portion of the moon's surface is illuminated; but it is only about the third day after the first quarter and at the corresponding phase of the waning moon, when the sun's rays, falling nearly horizontally, throw the details of this part of the surface into strong relief, that these appearances suggest the explanation now offered.

The other ridge answering to a terminal moraine occurs at the northern extremity of that magnificent valley which runs past the eastern edge of Rheita. This ridge is nearly semicircular, and is considerably elevated, both above the northern termination of the valley, and the general surface of the moon. It may be seen about four days after new and full moon; but the position of the observer with regard to the lights and shadows renders its appearance in the rays of the rising sun by far the most striking.

With regard to the probability of former glacial or even aqueous agency on the surface of the moon, difficulties of an apparently very formidable character present themselves. There is not only now no evidence whatever of the presence of water, in any one of its three forms, at the lunar surface, but, on the contrary, all seleniographic observations tend to prove its absence. Nevertheless the idea of former aqueous agency in the moon is by no means new; it was entertained by Gruithuisen and others. But if water at one time existed on the surface of the moon, whither has it disappeared? If we assume, in accordance with the nebular hypothesis, that the portions of matter composing respectively the earth and the moon once possessed an equally elevated temperature, it almost necessarily follows that the moon, owing to the comparative smallness of its mass, would cool much more rapidly than the earth; for whilst the volume of the moon is only about  $\frac{1}{49}$ th, its surface is nearly  $\frac{1}{13}$ th that of the earth.

This cooling of the mass of the moon must, according to all analogy, have been attended with contraction, which can scarcely

be conceived as occurring at considerable depths without the development of a cavernous structure in the interior. Much of this cavernous structure would doubtless communicate by means of fissures with the surface, and thus there would be provided an internal receptacle for the ocean, from the depths of which even the burning sun of the long lunar day would be totally unable to dislodge more than traces of aqueous vapour. Assuming the solid mass of the moon to contract on cooling at the same rate as granite, its refrigeration through only  $180^{\circ}$  F. would create cellular space equal to nearly  $14\frac{1}{2}$  millions of cubic miles, which would be more than sufficient to engulf the whole of the lunar ocean, supposing it to bear the same proportion to the mass of the moon as our own ocean bears to that of the earth.

If such be the present condition of the moon, we can scarcely avoid the conclusion that a liquid ocean can only exist upon the surface of a planet, so long as the latter retains a high internal temperature. The moon then becomes to us a prophetic picture of the ultimate fate which awaits our earth, when, deprived of an external ocean and its axial rotation reduced to a rate between monthly and annual\*, it shall revolve round the sun an arid and lifeless wilderness, each hemisphere alternately exposed to the protracted glare of a cloudless sun and plunged into the gloom of an arctic night.

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LIV. *On a New Method for the Direct Determination of the Specific Heat of Gases under Constant Volume.* By C. K. AKIN, Esq.†

SINCE the revival of the theory which considers heat as Energy or *vis viva* due to molecular motions, the exact knowledge, from direct experimental data, of the specific heats of gases under constant volume, which alone of all similar magnitudes represent what have been called *real* specific heats, has become more than ever important. The methods hitherto chiefly followed for the determination of the specific heats of gases under constant volume are exceedingly indirect; and whilst some of them, such as that of Dulong, involve hypotheses which

\* Mayer has proved that the action of the tides tends to arrest the motion of the earth upon its axis. And although the length of the terrestrial day has not increased by the  $\frac{1}{100}$ th part of a second since the time of Hipparchus, yet this fact obviously leaves untouched the conclusion to which Mayer's reasoning leads.

Since the above was written, Mr. James Croll has communicated to the *Philosophical Magazine* (vol. xxvii. p. 285) an elaborate paper on this influence of the tides.

† Communicated by the Author.

are contested, others, such as that of Clément and Desormes, proceed upon principles of which it is more than doubtful whether they have been formed with a sufficient regard to all the circumstances inherent in the case, the importance of several among the latter having only become felt since the resuscitation of the Baconian or Hermannian theory of heat\*. Whilst such is the case with regard to the more circuitous methods, other methods, in which it was intended to deduce the numerical values of the specific heats of gases under constant volume from experiments more nearly corresponding in their circumstances to the definition of the magnitudes they were designed to furnish, have failed to yield results possessing the confidence of philosophers†. Under these circumstances, and for the reasons above stated, it seems important that some new method should be devised, direct in theory and trustworthy in practice, capable of supplying the want of knowledge of the exact and real specific heats of gases in a manner which might command approbation. The real merits of any such method, of course, could only be tested by actual experience; but, although the method to be described in this paper, on the contrary, is only a scheme untried by experiment, it is yet hoped that its publication will not be without utility. For even if the plan, as a whole, should fail in practice in the case for which it has been more especially devised, it will yet be found to possess some new features, capable of wide and useful application elsewhere.

1. In order to clearness, we shall begin by illustrating the principle of the method here suggested, by the aid of a simply supposititious case. Let, then, a quantity of gas be supposed to be enclosed in a thin globular shell of a substance considered as impervious to, though a non-absorbent of heat. Let this substance, or the shell which is supposed to be made of it, further be assumed to be inextensible by pressure, though liable to disruption when opposed by a certain pressure whose magnitude shall be supposed to be known. Finally, let the terminals of a galvanic battery be taken to perforate the above shell in a manner not allowing of conduction by means of the shell (nor, of course, the efflux of the gas), but permitting of the passage of the electric current, derived from the battery, in a large platinum spiral suspended within the shell. If the battery be set in action, after a certain interval of time the temperature of the gas contained within the shell, we assume, shall become sufficiently great

\* As is well known, Bacon first defined heat as molecular motion; but it was Hermann, in his *Phoronomia* (p. 376), who first defined heat as molecular energy or *vis viva*.

† See, *e. g.*, M. Regnault's remarks in the second volume of his *Relation*, &c.



to cause, by the increased pressure of the gas, the disruption of the shell, when the whole experiment shall be concluded.

2. It will be easy now to show how, by means of the knowledge of the capacity and the pressure of disruption of the shell, the initial pressure and the initial temperature and weight of the gas, and with the aid of the laws of gaseous elasticity and of galvanic heating, the real specific heat of any gas subjected to an experiment like the above may be accurately deduced.

Let  $v$  be the volume common to both the shell and the gas,  $p$  the initial pressure, and  $t$  the initial temperature of the gas; then, by the law of Boyle (to which, however, it would be easy to substitute any other more accurate formula),

$$pv = R(a + t);$$

$R$  being a constant characteristic of the nature of the gas, and  $a$  the inverse coefficient of expansion. If  $P$  be the pressure of disruption, and  $T$  the corresponding temperature of the gas, then

$$Pv = R(a + T).$$

Hence, the increase of temperature owing to the heating of the gas,

$$\theta = T - t = (P - p) \frac{v}{R}, \quad \text{. . . . . (I.)}$$

where  $(T - t)$  is expressed by quantities all of which are known. It now remains to determine the quantity of heat by which this increase of temperature is produced. According to Mr. Joule's law, the quantity of heat given out by part of the circuit of a galvanic battery in the unit of time is

$$w = c . r . i^2,$$

where  $i$  denotes the intensity of the current,  $r$  the resistance of the part of circuit considered, and  $c$  a constant dependent on the unit of heat chosen. If the battery in the above experiment have acted during the interval of time  $\tau$ , then the amount of heat emitted by the spiral employed to heat the gas will be, the above formula being now applied to this spiral,

$$W = c . r . i^2 . \tau.$$

Let it now be supposed that the same battery and spiral be employed to heat, instead of the gas, a pound of water during the unit of time; the heat given out in this case will be immediately measured by the increase of temperature,

$$\theta_1 = c r_1 i_1^2,$$

$r_1$  and  $i_1$  being the magnitudes now taking the place of  $r$  and  $i$  in the former experiment. And since

$$W = \gamma \theta g,$$

where  $\gamma$  is the real specific heat of the gas, or its specific heat under constant volume, and  $g$  its weight, we obtain  $\gamma$ , as referred to the specific heat of water = 1,

$$\gamma = \frac{\theta_1}{\theta} \cdot \frac{r}{r_1} \cdot \frac{i^2}{i_1^2} \cdot \frac{\tau}{g}.$$

The magnitude  $\theta_1$  is measured directly by the thermometer, and  $\theta$  is known by means of formula (I.) Similarly,  $\tau$  may be measured directly by a timepiece, and  $i$  and  $i_1$  by means of a galvanometer. In order to determine also  $r$  and  $r_1$  accurately, the following method might be advantageously adopted. Having ascertained  $i$ , let the spiral be omitted from the circuit of the battery, but be replaced by a length of standard wire, of standard temperature, sufficient to reduce the intensity of the current to its original value  $i$ . If  $l$  be the length of standard wire required, then

$$r = l*.$$

In like manner  $r_1$  may be obtained,

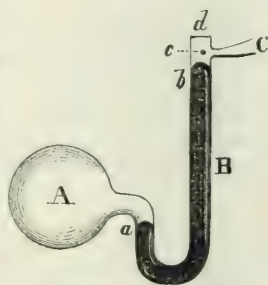
$$r_1 = l_1,$$

where  $l_1$  is known; so that in the end  $\gamma$  also becomes entirely known.

3. Having thus exemplified the principle of the method in an assumed case, we may now proceed to state in what manner the conditions implied in that example may be, very nearly at least, practically realized, or otherwise superseded by equivalent arrangements. The first condition which it is necessary to fulfil is, to preserve the volume of the gas constant during heating. For this purpose two slightly different means may be suggested.

First, let A in the adjoining figure represent a spherical vessel of rigid substance filled with gas, and B a siphon or manometric tube filled with mercury, from  $a$  up to  $b$ . If  $c$  be a very small hole in the tube near its upper extremity, and of sufficient tenuity, the column of mercury, in ascending through the very small range between  $b$  and  $d$ , will not be able to pass through the hole  $c$ , although it may be possible for air to pass freely through the hole. Hence, though the gas in A be heated, its volume will remain unaltered as soon as the mercury column has reached the top  $d$  of the tube B, which may be

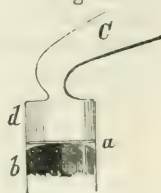
Fig. 1.



\* It is supposed in the above formula that the resistance of the remainder of the circuit is not perceptibly influenced by the interchange of the two resistances represented by  $r$  and  $l$ . This might practically be secured with sufficient approximation.

as little distant from *b* as may be desirable. When it is intended to stop the galvanic heating and to ascertain the final pressure of the gas, it would be only necessary to allow the ingress of air from some reservoir connected with a manometer and with C, its pressure being adjusted beforehand, by means of preliminary trials, so as to endow it with sufficient force to press down the mercury column from *d* to *b*. If this first means should not seem practicable, secondly, the tube B might have the shape shown in the adjoining figure 2, the tube C abutting at *d*, and  $\alpha$  representing a thin plate of some suitable substance lying on the top of the mercury column. In this case, again, the range of the mercury column may be made very small, the plate  $\alpha$  acting as a stopper; the volume of the gas will again remain constant, and the final pressure be capable of measurement, as before, by obvious means.

Fig. 2.



4. The next important condition implied in the preceding illustration is the caloric impermeability or neutrality of the enclosing vessel. In reality, this condition is impossible to fulfil, the heat engendered by the spiral being actually communicated in part only to the gas, whilst the remainder will be communicated to the vessel, and thence again, in part, to the medium surrounding the vessel. But, on the other hand, it is really not required to fulfil this condition, but only properly to distinguish and evaluate these several portions of heat. For this purpose, in the first place, the globe A might be immersed in a vessel containing water, whose temperature might be measured, and might also be taken to indicate the temperature of the globe A, though not of the gas contained in it. A better method, probably, would be to adapt to the globe in one or more places small thermo-couples connected with a galvanometer, by means of which its successive losses of heat by radiation and communication to the air, as well as its final temperature, might be determined after the manner resorted to in other similar cases.

5. Besides the difficulties which it has just been attempted to show how they might be overcome, there remain yet two others which require to be noticed. In the first place, the extensibility of the sphere A, both by increase of temperature and by interior pressure, has to be taken into account, in order to allow for the consequent variation of volume on the part of the gas, as also, what would be more difficult, for the loss of heat due to the resistance opposed by the elasticity of the sphere (and possibly also of the tube B) whilst expanded by pressure. In the next place, it would be necessary to determine also the heat communicated to the tube B and the mercury contained in it, so that



the heat really remaining with the gas may be correctly evaluated. It would be superfluous to attempt to indicate on this occasion the proper means by which the difficulties just explained might be either eluded or overcome—especially as, in the case of the first-mentioned, suitable expedients could only be discovered from actual trials, and would depend on the choice of substance of the globe, whilst with regard to the last-mentioned the means to be employed are sufficiently familiar. Thus much, however, may be stated, that none of the difficulties enumerated (and they seem, at least, all the more important ones) appear greater than what are habitually encountered in researches of this nature.

Oxford, March 1864.

#### LV. *On the Nature of Heat Vibrations.*

*By* MR. JAMES CROLL\*.

**I**N a most interesting paper on Radiant Heat, by Professor Tyndall, read before the Royal Society in March last, it is shown conclusively that the *period* of heat-vibrations is not affected by the state of aggregation of the molecules of the heated body; that is to say, whether the substance be in the gaseous, the liquid, or perhaps the solid condition, the tendency of its molecules to vibrate according to a given period remains unchanged. The force of cohesion binding the molecules together exercises no effect on the rapidity of vibration.

I had arrived at the same conclusion from theoretical considerations several years ago, and had also deduced some further conclusions regarding the nature of heat-vibrations, which seem to be in a measure confirmed by the experimental results of Prof. Tyndall. One of these conclusions was, that the heat-vibration does not consist in a motion of an aggregate mass of molecules, but in a motion of the individual molecules themselves. Each molecule, or rather we should say each atom, acts as if there were no other in existence but itself. Whether the atom stands by itself as in the gaseous state, or is bound to other atoms as in the liquid or the solid state, it behaves in exactly the same manner. The deeper question then suggested itself, viz. what is the nature of that mysterious motion called heat assumed by the atom? Does it consist in excursions across centres of equilibrium external to the atom itself? It is the generally received opinion among physicists that it does. But we think that the experimental results arrived at by Prof. Tyndall, as well as some others which will presently be noticed, are entirely hostile to such

\* Communicated by the Author.

an opinion. The relation of an atom to its centre of equilibrium depends entirely on the state of aggregation. Now if heat-vibrations consist in excursions to and fro across these centres, then the *period* ought to be affected by the state of aggregation. The higher the *tension* of the atom in regard to the centre, the more rapid ought its movement to be. This is the case in regard to the vibrations constituting sound. The harder a body becomes, or, in other words, the more firmly its molecules are bound together, the higher is the *pitch*. Two harp-cords struck with equal force will vibrate with equal force, however much they may differ in the rapidity of their vibrations. The *vis viva* of vibration depends upon the force of the stroke; but the rapidity depends, not on the stroke, but upon the tension of the cord.

That heat-vibrations do not consist in excursions of the molecules or atoms across centres of equilibrium, follows also as a necessary consequence from the fact that the real specific heat of a body remains unchanged under all conditions. All changes in the specific heat of a body are due to differences in the amount of heat consumed in molecular work against cohesion or other forces binding the molecules together. Or, in other words, to produce in a body no other effect than a given rise of temperature, requires the same amount of force, whatever may be the physical condition of the body. Whether the body be in the solid, the fluid, or the gaseous condition, the same rise of temperature always indicates the same quantity of force consumed in the simple production of the rise. Now if heat-vibrations consist in excursions of the atom to and fro across a centre of equilibrium *external to itself*, as is generally supposed, then the *real* specific heat of a solid body, for example, *ought to decrease with the hardness of the body*, because an increase in the strength of the force binding the molecules together would in such a case tend to favour the rise in the rapidity of the vibrations.

These conclusions not only afford us an insight into the hidden nature of heat-vibrations, but they also appear to cast some light on the physical constitution of the atom itself. They seem to lead to the conclusion that the ultimate atom itself is *essentially elastic*. For if heat-vibrations do not consist in excursions of the atom, then it must consist in alternate expansions and contractions of the atom itself. This again is opposed to the ordinary idea that the atom is essentially solid and impenetrable. But it favours the modern idea, that matter consists of a force of resistance acting from a centre.

LVI. *On Liquid Expansion.*By JOHN JAMES WATERSTON, *Esq.\**

[With a Plate.]

IN the Philosophical Magazine for June 1861 there is an account of a presumed law of liquid expansion which represents the proportionate differential of volume  $\frac{v dt}{dv}$  at a given

temperature  $t$ , as being simply proportional to the inverse of the distance of that temperature from a fixed upper limiting temperature  $\gamma$ . Expressed as an equation, this presumed law is

$\frac{v dt}{dv} \propto \gamma - t$ ; and the integral equation corresponding to this differential showed a seeming relation to the law of vapour-density

by an apparent constancy in the value of  $F$  (see above-mentioned paper, § 3), the product of the coefficient of saturated vapour density of a body by the exponent of the liquid density of the same body. I have for some time past been employed in testing this by means of M. Pierre's extensive series of experiments on liquid dilatation, taken in conjunction with those on vapour-tension by M. Regnault, lately published in his second volume of *Memoirs*. A large proportion of the liquids were the same in both, and chemically pure, so that the data are valuable for testing the law supposed to connect the volume of a liquid with the density of its saturated vapour at different temperatures, although not sufficient otherwise to test the law of liquid expansion by itself. But if  $F$  maintained its value at the lower range (that is, *under* the boiling temperature of the liquids), to which M. Pierre's researches were confined, as well as at the upper range, to which my observations in sealed tubes were chiefly directed, the integrity of the presumed law at all temperatures might with some certainty be inferred. Now, on projecting the coordinates of M. Regnault's vapour-tensions below the boiling-points of the respective liquids, according to the mode previously described (viz.  $\left(\frac{p}{t + 274}\right)^1$  laid off as ordinate to  $t$ ), I find that

the points for the most part range admirably in a line, and that a large proportion of those lines (including those of mercury and sulphur), when produced downwards, meet (below the axis of temperature) at the zero of gaseous tension  $-274^\circ$  (the apparent parallelism previously noted being actually a convergence).

This so far confirms the law of vapour-density, and gives a very accurate value for the coefficient  $h$  of each vapour respectively. The exponent  $\rho$  of the liquid, that ought to have a con-

\* Communicated by the Author.



stant ratio to this coefficient, depends on the value of the second differential of expansion; so that if the series of observations are not sufficiently correct or regular to give a tolerably approximate constancy of value to the second difference, they will fail to afford the test required.

The equation is

$$\frac{1}{\rho_1} = \frac{1}{t_1 - t_0} \left( \frac{v_1 dt}{dv_1} - \frac{v_0 dt}{dv_0} \right) \text{ and } \frac{1}{\rho} \times h = F = 504.$$

But instead of this value of  $F$  being maintained, after examining graphically and otherwise a considerable number of M. Pierre's tables of observations, I find that it varies from 280 to 380, and that on an average it is less at the lower than at the higher temperatures.

The discrepancy being so great, and always in one direction, has gradually undermined my confidence in the presumed law of liquid-expansion.

M. Drion's researches on the dilatability of volatile liquids, published in the *Annales de Chimie* for May 1859, I had unfortunately previously overlooked entirely, although two of the liquids chosen, viz. sulphurous acid and muriatic ether, are perhaps the best that could be selected for testing a theory, as they expand upwards of one-third of their volume on being heated to about  $130^\circ$ , and below this limit of temperature the difference in the march of the thermometers of air and mercury is too small to cause embarrassment. Also they are specifically distinct in their chemical relations—one having its elements  $O$  and  $S^{\frac{1}{2}}$  in a burnt condition, and the other,  $H^2C \cdot H^{\frac{1}{2}}Cl^{\frac{1}{2}}$ , having one part burnt ( $H^{\frac{1}{2}}Cl^{\frac{1}{2}}$ ), and the other part ( $H^2C$ ) unburnt.

On projecting these observations graphically, they are found to be wonderfully free from irregularities; so I trust to not having over-estimated the importance of the results obtained from their graphical analysis, which is presented in the accompanying chart (Plate VII.).

Instead of the *proportionate* expansion for  $1^\circ$  being inversely as the temperature below an upper fixed limit, it will be remarked that the evidence is in favour of the *absolute* expansion for  $1^\circ$  having this ratio. Both values are laid down, the inverse of the *proportionate* expansion for  $1^\circ$  ranging along  $aa$  and  $cc$ , the inverse of the *absolute* expansion for  $1^\circ$  ranging along  $bb$  and  $dd$ . By employing a straight edge, the exact accordance or discordance of the observations with either hypothesis may be ascertained.

According to the previous hypothesis (the *proportionate*), the points in  $aa$  should not only range in a straight line, but that line ought to be  $SS$ , or at least parallel to it, if  $F$  held the con-

stant value 504. Also the points in *cc* should range in *MM*. They do so approximately, it will be remarked, in the upper part; and this perhaps is the cause of the mistaken induction, as it was the upper part that was chiefly relied upon in my sealed-tube observations.

The following are M. Drion's observations from which the muriatic ether points were computed (*Ann. de Chim.* vol. lvi. p. 21):—

	Tempera- ture.	Volume.		Tempera- ture.	Volume.
No. 1	0	1.0000	No. 8	83	1.1647
2	12.6	1.0195	9	93.5	1.1946
3	26.4	1.0428	10	103	1.2248
4	35.0	1.0586	11	112	1.2567
5	45.5	1.0787	12	120.5	1.2908
6	59.0	1.1067	13	128	1.3249
7	71.5	1.1354	14	133	1.3507

The differentials are computed from the couples 1 and 4, 2 and 5, 3 and 6, &c.; *e. g.* for the first couple  $dt=35^{\circ}$ ,  $dv=.0586$ , and  $t$ , the middle temperature, is  $17^{\circ}.5$ ; so  $\frac{dt}{dv}=597$ , which is the ordinate to  $17.5$  for the first point in the line  $dd$ . The corresponding first point in the line  $cc$  is obtained by multiplying the last, viz. 597, by 1.029, the mean volume between Nos. 1 and 4. This gives  $\frac{vdt}{dv}=615$  as the ordinate to be laid off to  $17.5$ , the mean temperature as before. The following Table is thus computed:—

#### Muriatic Ether.

Couples.	$t$ .	$\frac{dt}{dv}$ .	$\frac{vdt}{dv}$ .
1 & 4	17.5	597	615
2 " 5	29.0	559	587
3 " 6	42.7	509	545
4 " 7	53.3	474	520
5 " 8	64.2	436	488
6 " 9	76.2	392	450
7 " 10	87.2	350	413
8 " 11	97.5	315	381
9 " 12	107.0	281	348
10 " 13	115.5	250	319
11 " 14	122.5	223	290

The following are M. Drion's observations from which the

points for sulphurous acid were computed (*Ann. de Chim.* vol. lvi. p. 35):—

	Tempera- ture.	Volume.		Tempera- ture.	Volume.
No. 1	0°	1·0000	No. 8	82·5	1·2058
2	12·6	1·0230	9	91·0	1·2393
3	26·4	1·0509	10	100·0	1·2796
4	35	1·0698	11	108·5	1·3238
5	49·5	1·1048	12	115·5	1·3664
6	62·5	1·1403	13	122·0	1·4133
7	72·5	1·1710	14	126·8	1·4538

The computed differentials are in the following Table:—

### Sulphurous Acid.

Couples.	$t$ .	$\frac{dt}{dv}$ .	$\frac{v dt}{dv}$ .
1 & 4	17·5	498	518
2 " 5	31·0	452	481
3 " 6	44·5	402	442
4 " 7	53·7	371	416
5 " 8	66·0	327	377
6 " 9	76·7	288	342
7 " 10	86·2	254	312
8 " 12	99·0	205	264
9 " 14	108·9	167	225

By laying a straight edge along the line  $bb$ , it will be found that all the points except the lowest and two highest are very exactly in a straight line, which, produced downwards, crosses the zero vertical at 564 on the scale, and which, produced upwards ( $\beta\beta$ ), intersects the axis of temperature in 157·5, which is  $\gamma_s$ , the upper limiting temperature of sulphurous acid. Now

$$\frac{564}{157\cdot5} = 3\cdot581 = P_s,$$

and the integral of the differential equation gives

$$\text{hyp. log } \gamma_s - \text{hyp. log } (\gamma_s - t) = P_s(v - 1),$$

which is reduced to common logarithms by dividing  $P_s$  by 2·3026.

A straight edge laid along  $dd$  shows that the points range tolerably in a line  $\delta\delta$  that passes through 663 on scale of zero vertical and 187° of the axis, which is  $\gamma_m$ , the upper limiting temperature of muriatic ether, and  $\frac{663}{187} = 3\cdot545 = P_m$ .

The faint dotted lines  $\alpha\alpha$  and  $\epsilon\epsilon$  are the computed *loci* of



$\frac{vdt}{dv}$ , corresponding to  $\beta\beta$  and  $\delta\delta$  straight lines, assumed to be the loci of  $\frac{dt}{dv}$  at the extreme upper part of the range.

It will be remarked that  $P_s = P_m$  very nearly in these two liquids; but this must be an accidental coincidence, as the numerical value of  $P$  obviously varies inversely as the numerical value which may be arbitrarily given to the volume at  $0^\circ$ .

There are various considerations with respect to molecular volume, and the relation of  $h$  and  $\gamma$  reckoned from the zero of gaseous tension, that it would be proper to enter upon if it were decided that this is the true general law of liquid-expansion; but, as in the former case, we may be too hasty in this. I only hope that what is here set forth may lead to further inquiry. The simplicity of the new differential equation is greater than the previous one—a material point in its favour, since true progress and simplicity seem always to go together.

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*Note.*

If the true theory of liquid-expansion has now been discovered, other symmetrical relations are likely to appear, and more especially in those bodies of similar composition, as the hydrochloric, hydrobromic, and hydriodic ethers, which have fortunately been experimented upon both by M. Regnault and by M. Pierre.

The following will serve to show how far their observations strictly respond to certain relations presumed to exist between the constants of the thermo-molecular lines of those bodies.

The vapour-density lines of the three ethers, (1)  $\text{CH}^2 \cdot \text{H}^{\frac{1}{2}} \text{Cl}^{\frac{1}{2}}$ , (2)  $\text{CH}^2 \cdot \text{H}^{\frac{1}{2}} \text{Br}^{\frac{1}{2}}$ , and (3)  $\text{CH}^2 \cdot \text{H}^{\frac{1}{2}} \text{I}^{\frac{1}{2}}$ , projected from Regnault's vapour-tensions (dynamic series) below atmospheric pressure, give the following values for  $g$  and  $h$  in the general equation

$$\left\{ \frac{t-g}{h} \right\}^6 = \frac{p}{t},$$

in which both  $t$  and  $g$  are reckoned from  $-274^\circ$ , the zero of gaseous tension.

	$g.$ °	$h.$	$\frac{h}{g}.$	
(1)	117·0	143·7	1·228	} Mean 1·237.
(2)	125·9	161·0	1·277	
(3)	145·1	175·1	1·207	

General mean of 16 liquids is also 1·237.

It will be observed that the numbers in the two columns follow the same ratio very nearly, showing that the lines all meet

at the zero-point of gaseous tension,—a very significant fact in molecular physics. To this central point or ether-node (as it may be termed) converge the lines of the greater number of the vapours experimented upon by M. Regnault. But water, alcohol, and sulphuric acid hydrates are notable exceptions; and there are others, viz. pyroxylic spirit, pyroxylic ether, sulphurous acid, and oxalic ether, which seem to trend towards the water-node.

The liquid-expansion lines (as those on Plate marked  $\frac{dt}{dv}$ ) of the same three ethers, projected from M. Pierre's observations of expansion, give the following values for  $G$ , the upper limiting temperature reckoned from the zero of gaseous tension (or  $G=274+\gamma$ ), and the molecular volumes at the same zero  $\mu = \frac{V.D.}{\text{sp. gr.}}$ , or quotient of the vapour-density of the ether by its specific gravity at  $-274^\circ$ , computed by means of the equation

$$\{\log \gamma - \log (\gamma + 274)\} \frac{2 \cdot 302}{P} = (v-1).$$

	$G$ .	$\mu$ .	$\frac{G}{h}$ .	$\frac{h}{\mu}$ .	
(1)	460.0	26.0	3.22	55.3	} Mean 56.5.
(2)	534.4	27.8	3.32	57.9	
(3)	597.4	31.1	3.41	56.3	

It will be remarked that the numbers in columns  $G$  and  $\mu$  are nearly proportional to those above in columns  $h$  and  $g$ . The same equality of ratio is found in other cases.

There are also certain apparent relations of harmony in the absolute measure of liquid cohesion of a molecule as deduced from the latent heat of its vapour. Thus the latent heat of a molecule of water is exactly the same as that of a molecule of alcohol at  $0^\circ$ .

The latent heat of a molecule of hydrochloric ether is to that of hydriodic ether as the initial molecular volumes at  $-274^\circ$ ?

Edinburgh, March 16, 1864.

LVII. *On the Dynamics of the Galvanic Battery.*

By Dr. A. W. WILLIAMSON, F.R.S.

To Dr. Francis, F.L.S. &c.

DEAR SIR,

IT appears to be incumbent on me to say a few words in reply to Mr. James Napier's letter in a recent Number of the Philosophical Magazine, impugning some statements which I  
*Phil. Mag. S. 4. Vol. 27. No. 183. May 1864.* 2 A

made in an evening discourse delivered before the British Association at Newcastle, "On the Dynamics of the Galvanic Battery."

Mr. Napier quotes from my discourse a paragraph illustrating, in the very simple case of aqueous hydrochloric acid, some general facts respecting the atomic motions which occur in electrolysis. He does not deny the continuous evolution of hydrogen at the negative pole, and of chlorine at the positive pole; nor does he point out any fault in my reasoning upon these well-known facts. But he proceeds to apply what he mistakes for the principle of my explanation to an example of his own selection, and finding himself at fault, endeavours to throw the blame on me.

His experiment is the passage of a galvanic current through a solution of sulphate of copper divided by a porous diaphragm into two equal portions, one copper pole being immersed in one portion, while the other copper pole is immersed in the other portion. He asserts that, according to my "assumed theory," there must be through this diaphragm a transfer of copper towards the negative pole equal to the transfer of the chlorous element towards the positive pole; whereas experiment shows an exhaustion of all the salt in the negative division, and a corresponding accumulation in the positive division.

Not only have I made no statement implying an equal velocity of movement of the chlorous and basylous atoms, but I have yet to learn that the existence of such equality would in any degree favour my argument, or that the absence of such equality would militate against my argument.

I said that the atom of hydrogen which has given up its chlorine at the positive pole combines with chlorine from the next molecule of hydrochloric acid; and this is equally true whether it moves towards the chlorine, or whether the chlorine moves towards it. I certainly never took it for granted, as Mr. Napier imagines, that the atoms make equal advances towards each other. Indeed Mr. Napier quotes my very words: "It is unnecessary to discuss at present the particular manner in which this atomic motion takes place, but there is no doubt of the fact," &c.

I remain,

Yours very truly,

ALEX. W. WILLIAMSON.

University College, London,  
April 12, 1864.



LVIII. *Differential Equations of the First Order.*

By CHARLES JAMES HARGREAVE, LL.D. Dublin, F.R.S.\*

*Introductory Remarks.*

1. **T**HE object of this memoir is to call attention to a defect in the treatment of differential equations of the first order, and of the second or any higher degree, and to supply that defect, so far as my researches enable me.

With regard to this class of equations, the only general process of solution given in elementary works consists in resolving the equation into as many equations of the first degree as it will admit of, and then solving each of these equations of the first degree by any method which may be practicable for each of them. An attempt is then made to combine all these distinct solutions into one, by a process which I conceive to be an illusory one. The result of this method, therefore, is, not the solution of a differential equation of the  $n$ th degree, but the separate solution of  $n$  equations of the first degree, which are perfectly distinct, and incapable of being connected by any process arising out of this method. In practice, equations of the second and higher degrees are not solved by this method—except a few forms prepared for the purpose in such a manner that the radical operations are capable of being actually performed, so that we obtain equations of the first degree free from radicals. Of this character are ( $p$  being  $\frac{dy}{dx}$ ),

$$p^2 - a^2 y^2 = 0 \text{ (Boole, Diff. Eq. p. 116),}$$

$$p^3 - (x^2 + xy + y^2)p^2 + (x^3y + x^2y^2 + xy^3)p - x^3y^3 = 0$$

(Gregory, Ex. 328),

$$(a^2 - x^2)p^3 + bx(a^2 - x^2)p^2 - (p + bx) = 0 \text{ (Idem),}$$

which admit of division into two or three simple equations not containing radicals, and each easily integrable. The results of these separate integrations cannot, however, be connected together, except by the fallacious process of making the three arbitrary constants all equal, which is a logical absurdity. Take, for example, the second of the above instances, and resolve it into

$$p - x^2 = 0, \quad p - xy = 0, \quad p - y^2 = 0.$$

The three separate integrals are, or rather may be,

$$c_1 - \left(y - \frac{1}{3}x^3\right) = 0, \quad c_2 - \left(y - \frac{x^2}{2}\right) = 0, \quad c_3 - \left(y + \frac{1}{x}\right) = 0;$$

\* Communicated by the Author.

each of which admits of an infinite variety of forms by reason of each constant being arbitrary. If, therefore, we profess to make the constants equal, we merely transfer the arbitrariness to the functions, and obtain

$$(c-f_1(y-\frac{x^3}{3}))(c-f_2(y-\frac{x^2}{2}))(c-f_3(y+\frac{1}{x}))=0$$

as the complete primitive. If we proceed further, and choose to make the three functional forms identical, we have still made no advance towards connecting the three solutions; for the arbitrariness is merely transferred again from the functional symbols to the respective arguments governed by them. These arguments are not, as appears to be assumed, definite functions, but each of them is just as arbitrary as if it were governed by an arbitrary symbol. We have no more right to place the solution of

$$p-x^2=0$$

in the form

$$c=y-\frac{x^3}{3},$$

than in any other form such as

$$c=\log(y-\frac{1}{3}x^3),$$

$$c=e^y e^{-\frac{x^3}{3}},$$

$$c=(y-\frac{x^3}{3})^n + a(y-\frac{x^3}{3})^{n-1} + \dots,$$

and so on indefinitely, the particular argument actually obtained being the result of accident—that is, depending upon the particular integrating factor made use of in order to obtain it. It is of course quite obvious that when the equation is once resolved into  $n$  distinct equations, no connexion can exist between the integrating factors which we may choose to employ for their integration, since they are  $n$  totally distinct equations.

This general method, therefore, is fallacious, if it professes to do more than to integrate the  $n$  distinct equations into which the differential equation of the  $n$ th degree is divisible; and if it does not profess to do more than this, it is useless.

On this subject, I refer to Professor Boole's excellent *Treatise*, chap. vii. sect. 3. In that section, the complete primitive is supposed to admit of the definite form

$$(c-V_1)(c-V_2) \dots (c-V_n)=0.$$

We may, however, substitute  $f_1 V_1$  for  $V_1$ ,  $f_2 V_2$  for  $V_2$ , and so on

(where  $f_1, f_2, \dots, f_n$  are all arbitrary and unconnected), without invalidating the argument of that section. There cannot, therefore, be any definite connexion (of equality or otherwise) between the different arbitrary functions or constants thus introduced.

2. In what manner, then, must the constants be introduced so as to admit of their being definitely connected? A partial answer to this question may be found in every case in which an equation of the  $n$ th degree is integrable without resolution. I have already observed that equations of the  $n$ th degree are not in practice integrated by resolution and separate integration, but by special methods of a totally different character, by some process, in short, which connects the separate integrals in a manner which can be neither mistaken nor avoided. This circumstance is shown in a very clear and remarkable manner in the special mode of solution which is employed for the integration of equations of the  $n$ th degree of the form

$$y = xfp + \phi p,$$

which includes Clairaut's form, and also includes, in effect, homogeneous equations. It would be idle to attempt to solve such an equation as this by resolution with regard to  $p$ , and then  $n$  separate searches after a complete primitive, the results to be afterwards connected together in a fortuitous and unsystematic manner. The solution in fact is effected quite otherwise—by a process, that is, which enables us to find an equation between  $p$ ,  $x$  and an arbitrary constant. At this point, the necessary connexion between the  $n$  factors of the result presents itself without the possibility of mistake; for since  $p$  has  $n$  values,  $p_1, p_2, \dots, p_n$ , which are indistinguishable *inter se*, it is manifest that, whatever may be the form or character of the result in other respects, it must be a symmetrical function of these  $n$  values of  $p$ . The process which leads to this symmetrical result is simply the differentiation of the original equation, which gives

$$p - fp = (xf'p + \phi'p) \frac{dp}{dx},$$

or

$$\frac{dx}{dp} = \frac{xf'p + \phi'p}{p - fp},$$

a linear differential equation between  $x$  and  $p$  of the first order, and therefore integrable in the form

$$c = xFp + \Phi p.$$

And since  $p$  has  $n$  values, which are the  $n$  roots of

$$xfp + \phi p - y = 0,$$



the final result must be

$$(c - (xFp_1 + \Phi p_1))(c - (xFp_2 + \Phi p_2)) \dots (c - (xFp_n + \Phi p_n)) = 0,$$

an equation of the  $n$ th degree in  $c$ , the coefficient of each power of  $c$  being a symmetrical function of the  $n$  values of  $p$ . This result contains only one element of arbitrariness; whereas if we had proceeded by resolution and separate integration, we should have introduced  $n$  unconnected constants or arbitrary functions.

3. These two kinds of solution—the solution at large, and the connected or systematic solution, as they may be called—may be illustrated by any example which admits of being dealt with both ways. Take, for example, the equation of the second degree,

$$p^2 - p \frac{y}{x} + \frac{m}{x} = 0.$$

Its connected solution by a special method (Clairaut's) is

$$c^2 - c \frac{y}{x} + \frac{m}{x} = 0.$$

This form, though not unique, admits of one element of variation only. We may write for  $c$  any function of  $c$ , and so obtain an infinite series of forms such as

$$c^2 - c \left( \frac{y^2}{x^2} - \frac{2m}{x} \right) + \frac{m^2}{x^2} = 0,$$

$$c^2 - cy \left( \frac{1}{x} + \frac{1}{m} \right) + \frac{m}{x} + \frac{x}{m} + 2 = 0,$$

and so on.

If we proceed by way of resolution, we obtain

$$p = \frac{1}{2} \left( \frac{y}{x} \pm \sqrt{\frac{y^2}{x^2} - \frac{4m}{x}} \right).$$

The integration of one of these will give *some* form of

$$c = f_1 \left( \frac{y}{2x} + \sqrt{\frac{y^2}{4x^2} - \frac{m}{x}} \right);$$

and that of the other will give *some* form of

$$c = f_2 \left( \frac{y}{2x} - \sqrt{\frac{y^2}{4x^2} - \frac{m}{x}} \right);$$

but the two equations and their resulting forms are entirely unconnected, and the solution so obtained necessarily involves two arbitrary elements.

4. My object in this paper is to endeavour to solve generally equations of the second and higher degrees by some systematic process, so as to discover their connected primitive,—the prac-

tical effect of this connexion being to produce the final result in the form of an equation in  $c$  of the same degree as the differential equation is in  $p$ . With regard to the latter equation, I assume that its coefficients are rational, or rather unambiguous in value. Should these coefficients contain radicals or ambiguous forms, they must be expelled before we can determine the real degree of the equation in  $p$ . We cannot, however, assume that the coefficients of the complete primitive in  $c$  will also be rational, the process of integration being apt to produce transcendental forms in lieu of rational algebraic ones.

The investigation which follows will, I think, be found to solve this problem completely for equations of the second degree; and it exhibits some processes of solution which possess an independent interest.

### *Proposed Theory.*

5. Having indicated the object in view, I now proceed to the consideration of differential equations of the first order, beginning with those of the second degree. These equations are of the form

$$p^2 - 2\phi_1 p + \phi_2 = 0,$$

$\phi_1$  and  $\phi_2$  being unambiguous functions of  $x$  and  $y$ .

It is obvious that in general a primitive of the form

$$c^2 - 2c\psi_1 + \psi_2 = 0$$

will give, by the elimination of  $c$ , a differential equation of the form above written, in which  $\phi_1$  and  $\phi_2$  are easily obtained from  $\psi_1$  and  $\psi_2$ . This proposition, however, although true in general, admits of a particular exception. If  $\psi_2$  happen to be a function of  $\psi_1$  (say  $f\psi_1$ ), we merely obtain by differentiation

$$2c = f',$$

so that  $c$  does not contain  $p$ ; and the requisite elimination is not possible.

The primitive may be placed conveniently in the form

$$(cf)^2 - 2(cf)\psi + 1;$$

which gives for  $c$  the two values

$$c = \frac{1}{f} (\psi \pm \sqrt{\psi^2 - 1});$$

and this (remembering that  $c$  is arbitrary, and that therefore any function of  $c$  may be written for  $c$ ) admits of being placed in the form

$$c = \mu \pm \varpi$$

in an infinite variety of ways. Thus we may make  $\mu = \frac{\psi}{f}$ , and

$\varpi = \frac{1}{f} (\psi^2 - 1)^{\frac{1}{2}}$ ; or we may make  $\mu = \log f$ , and

$$\varpi = \log(\psi + \sqrt{\psi^2 - 1}),$$

and so on.

In this form of the primitive  $p$  or  $\frac{dy}{dx}$  has the two values,

$$p_1 = -\left(\frac{d\mu}{dx} + \frac{d\varpi}{dx}\right) \div \left(\frac{d\mu}{dy} + \frac{d\varpi}{dy}\right),$$

$$p_2 = -\left(\frac{d\mu}{dx} - \frac{d\varpi}{dx}\right) \div \left(\frac{d\mu}{dy} - \frac{d\varpi}{dy}\right),$$

which therefore by hypothesis are the roots of

$$p^2 - 2\phi_1 p + \phi_2 = 0.$$

This gives the two equations

$$p_1 + p_2 = 2\phi_1,$$

$$p_1 p_2 = \phi_2;$$

or

$$\phi_1 \left( \left( \frac{d\mu}{dy} \right)^2 - \left( \frac{d\varpi}{dy} \right)^2 \right) + \frac{d\mu}{dy} \frac{d\mu}{dx} - \frac{d\varpi}{dy} \frac{d\varpi}{dx} = 0,$$

$$\phi_1 \left( \left( \frac{d\mu}{dx} \right)^2 - \left( \frac{d\varpi}{dx} \right)^2 \right) + \phi_2 \left( \frac{d\mu}{dy} \frac{d\mu}{dx} - \frac{d\varpi}{dy} \frac{d\varpi}{dx} \right) = 0;$$

which may be put into the symmetrical form

$$\frac{du}{dy} \left( \frac{d\mu}{dx} + \phi_1 \frac{d\mu}{dy} \right) - \frac{d\varpi}{dy} \left( \frac{d\varpi}{dx} + \phi_1 \frac{d\varpi}{dy} \right) = 0,$$

$$\frac{d\mu}{dx} \left( \phi_1 \frac{d\mu}{dx} + \phi_2 \frac{d\mu}{dy} \right) - \frac{d\varpi}{dx} \left( \phi_1 \frac{d\varpi}{dx} + \phi_2 \frac{d\varpi}{dy} \right) = 0.$$

Let

$$\frac{d\varpi}{dy} \div \frac{d\mu}{dy} = t_y; \quad \frac{d\varpi}{dx} \div \frac{d\mu}{dx} = t_x, \quad \frac{d\mu}{dy} \div \frac{d\mu}{dx} = z.$$

These equations then are

$$(1 + \phi_1 z) - t_y(t_x + \phi_1 t_y z) = 0,$$

$$(\phi_1 + \phi_2 z) - t_x(\phi_1 t_x + \phi_2 t_y z) = 0.$$

Eliminate  $t_x$  and  $t_y$  successively, and we obtain ( $R^2$  being written for  $\phi_1^2 - \phi_2$ ),

$$(t_y^2 - 1)((1 + \phi_1 z)^2 - R^2 z^2 t_y^2) = 0,$$

$$(t_x^2 - 1)((\phi_1 + \phi_2 z)^2 - R^2 t_x^2) = 0.$$

Now neither  $t_y^2 - 1$  nor  $t_x^2 - 1$  can be zero, inasmuch as  $\varpi - \mu$



must be a function of  $x$  and  $y$ . Therefore

$$1 + \phi_1 z = \pm R z t_y,$$

$$\phi_1 + \phi_2 z = \pm R t_x;$$

or

$$\frac{d\mu}{dy} + \phi_1 \frac{d\mu}{dy} = \pm R \frac{d\varpi}{dy},$$

$$\phi_1 \frac{d\mu}{dx} + \phi_2 \frac{d\mu}{dy} = \pm R \frac{d\varpi}{dx}.$$

As these equations must be symmetrical with regard to  $\mu$  and  $\varpi$ , as are the equations from which they are derived,  $R$  must have a positive sign in one of them, and a negative sign in the other; and the results may be expressed in either of the two equivalent systems,

$$\frac{d\mu}{dy} = \frac{1}{R} \left( \frac{d\varpi}{dx} + \phi_1 \frac{d\varpi}{dy} \right),$$

$$\frac{d\mu}{dx} = -\frac{1}{R} \left( \phi_1 \frac{d\varpi}{dx} + \phi_2 \frac{d\varpi}{dy} \right),$$

and

$$\frac{d\varpi}{dy} = \frac{1}{R} \left( \frac{d\mu}{dx} + \phi_1 \frac{d\mu}{dy} \right),$$

$$\frac{d\varpi}{dx} = -\frac{1}{R} \left( \phi_1 \frac{d\mu}{dx} + \phi_2 \frac{d\mu}{dy} \right),$$

the intrinsic sign of  $R$  being immaterial.

6. These are the equations which for all quadratic differential forms exhibit the connexion which I endeavoured to explain in the introductory observations. They constitute a species of condition which we must satisfy before we can return to the complete primitive in a rational and connected form.

Considered in the light of a condition, the two equations of the system are really one. For remembering that  $c$  is  $\mu \pm \varpi$  (or a function of it), we have from them

$$R \frac{dc}{dy} = \frac{d\varpi}{dx} + (\phi_1 \pm R) \frac{d\varpi}{dy},$$

$$-R \frac{dc}{dx} = (\phi_1 \mp R) \frac{d\varpi}{dx} + \phi_2 \frac{d\varpi}{dy};$$

whence

$$-\frac{dc}{dx} \div \frac{dc}{dy}, \text{ or } p = \phi_1 \pm R,$$

which is the original differential equation in its resolved form.

7. This connecting system lies at the root of the theory; and I may be excused for giving another proof of its existence.

The differential equation being

$$p^2 - 2\phi_1 p + \phi_2 = 0,$$

and the primitive being

$$c^2 - 2\psi_1 c + \psi_2 = 0,$$

it is easy to find  $f$  and  $\chi$  so that the primitive may admit of the form ( $v$  being  $\phi_1 \div \phi_2^{\frac{1}{2}}$ )

$$(cf + \chi)^2 - 2v(cf + \chi) + 1 = 0;$$

in which case  $p$  and  $(cf + \chi)\phi_2^{\frac{1}{2}}$  are identical. This is

$$c^2 f^2 - 2cf\lambda + \lambda^2 - (v^2 - 1) = 0 \quad (\lambda \text{ being } v - \chi),$$

which (again altering the form of  $f$ ) may be written

$$c^2 f^2 - 2cf \frac{\lambda}{\sqrt{\lambda^2 - v^2 + 1}} + 1 = 0,$$

$$cf \text{ now being } \frac{1}{\sqrt{\lambda^2 - v^2 + 1}} \left( \frac{p}{\sqrt{\phi_2}} - v + \lambda \right);$$

or, finally,

$$c^2 f^2 - 2cf\psi + 1 = 0, \text{ where } cf = \psi \left( 1 + \frac{1}{\lambda} \left( \frac{p}{\sqrt{\phi_2}} - v \right) \right),$$

and (calling  $\frac{f'}{f} = \mu'$ )

$$cf' = \mu' \psi \left( 1 + \frac{1}{\lambda} \left( \frac{p}{\sqrt{\phi_2}} - v \right) \right).$$

Now differentiate, divide by  $2cf$ , and eliminate  $c$  by writing for  $cf'$  its equivalent, and we have

$$\left( \frac{p}{\sqrt{\phi_2}} - v \right) \frac{\psi}{\lambda} \mu' - \psi' = 0,$$

or

$$p^2 \frac{d\mu}{dy} + p \left( \frac{d\mu}{dx} - \phi_1 \frac{d\mu}{dy} - \frac{\lambda}{\psi} \sqrt{\phi_2} \frac{d\psi}{dy} \right) - \left( \phi_1 \frac{d\mu}{dx} + \frac{\lambda}{\psi} \sqrt{\phi_2} \frac{d\psi}{dx} \right) = 0.$$

Comparing this with the given form of the differential equation we have,  $\varpi$  being  $\log(\psi + \sqrt{\psi^2 - 1})$ ,

$$\frac{d\mu}{dx} + \phi_1 \frac{d\mu}{dy} = \frac{\lambda}{\psi} \sqrt{\phi_2} \frac{d\psi}{dy} = \frac{R}{(\psi^2 - 1)^{\frac{1}{2}}} \frac{d\psi}{dy} = R \frac{d\varpi}{dy},$$

$$\phi_1 \frac{d\mu}{dx} + \phi_2 \frac{d\mu}{dy} = \frac{\lambda}{\psi} \sqrt{\phi_2} \frac{d\psi}{dx} = \frac{R}{(\psi^2 - 1)^{\frac{1}{2}}} \frac{d\psi}{dx} = -R \frac{d\varpi}{dx},$$

which are the same equations as before obtained, but without irrelevant factors.

8. When the original differential equation is resolved into

$$p = (\phi_1 \pm R),$$

or

$$dy = (\phi_1 \pm R)dx,$$

the two integrating factors are  $\frac{dc}{dy}$  derived from  $c = \mu + \varpi$ , and  $\frac{dc}{dy}$  derived from  $c = \mu - \varpi$ ; and these are

$$\frac{1}{R} \left( \frac{d\varpi}{dx} + (\phi_1 \pm R) \frac{d\varpi}{dy} \right),$$

or similar functions of  $\mu$ . Integrating factors having this relation may be called the connected integrating factors.

9. If we take either of the two equivalent fundamental systems, and differentiate the first equation with regard to  $x$ , and the second with regard to  $y$ , and equate the two results, we obtain the linear partial differential equation of the second order,

$$\frac{d^2z}{dx^2} + 2\phi_1 \frac{d^2z}{dx dy} + \phi_2 \frac{d^2z}{dy^2} + P \frac{dz}{dx} + Q \frac{dz}{dy} = 0;$$

in which we have written

$$P \text{ for } R \left( \frac{d\left(\frac{1}{R}\right)}{dx} + \frac{d\left(\frac{\phi_1}{R}\right)}{dy} \right),$$

and

$$Q \text{ for } R \left( \frac{d\left(\frac{\phi_1}{R}\right)}{dx} + \frac{d\left(\frac{\phi_2}{R}\right)}{dy} \right).$$

As this partial equation is satisfied by  $z = \varpi$ , or by  $z = \mu$ , the complete solution of  $p^2 - 2\phi_1 p + \phi_2 = 0$  is thus seen to depend on the discovery of any solution whatever of this partial equation. If we can find any function of  $x$  and  $y$ , or either of them, which proves to be a particular solution of this partial equation, and call it  $\varpi$ , we then obtain  $\mu$  from the fundamental system; and it is of course also a solution of the partial. Our object in the particular case is then accomplished.

Conversely (to digress for a moment from the main subject), if the complete solution of

$$p^2 - 2\phi_1 p + \phi_2 = 0$$

is known in the form

$$c = \mu \pm \varpi,$$

then the general solution of the partial equation must be

$$z = f_1(\mu + \varpi) + f_2(\mu - \varpi).$$



I have subjoined to this paper a note containing an independent discussion of this somewhat remarkable partial equation.

10. There are, it is obvious, some cases in which the partial equation admits of a particular solution being easily found, so as to be available for the solution of the ordinary differential equation; and it may also in many cases be solved under some restrictive hypothesis, the result indicating whether the hypothesis is a legitimate one. In fact, it follows the analogy of the partial equation of the first order which we make use of in the integration of equations of the first degree. (See Boole's *Diff. Eq.* chap. v.)

11. Before proceeding to apply these formulæ to the solution of examples, I think it useful to call attention to the result of eliminating  $R$  (as it appears explicitly) from the two equations of the fundamental system. If we make

$$\Pi = \frac{d\varpi}{dx} \div \frac{d\varpi}{dy}, \quad M = \frac{d\mu}{dx} \div \frac{d\mu}{dy},$$

the elimination of  $R$  gives

$$M\Pi + \phi_1(M + \Pi) + \phi_2 = 0, \text{ or } (M + \phi_1)(\Pi + \phi_1) = R^2.$$

If therefore we know in what manner  $x$  and  $y$  enter into  $\varpi$ , we can ascertain in what manner they enter into  $\mu$ , without inquiring what functions of these forms  $\varpi$  and  $\mu$  respectively are.

Thus suppose we have  $\phi_1 = a$ ,  $\phi_2 = \frac{y^2}{x^2}$ , and therefore  $R^2 = a^2 - \frac{y^2}{x^2}$ ;

if we know that  $\Pi$  is  $-\frac{y}{x}$  in consequence of  $\varpi$  being a function of  $\frac{y}{x}$ , we infer at once that  $M$  is  $\frac{y}{x}$ , and therefore that  $\mu$  is a function of  $xy$ .

#### *Modes of Solution, and Examples.*

12. The partial equation expressed in the usual notation is

$$r + 2\phi_1 s + \phi_2 t + Pp + Qq = 0,$$

$$P \text{ being } \frac{d\phi_1}{dy} - \left( \frac{d(\log R)}{dx} + \phi_1 \frac{d(\log R)}{dy} \right),$$

and

$$Q \text{ being } \frac{d\phi_1}{dx} + \frac{d\phi_2}{dy} - \left( \phi_1 \frac{d(\log R)}{dx} + \phi_2 \frac{d(\log R)}{dy} \right).$$

There are two cases in which a particular solution of this partial equation presents itself spontaneously. First, if  $P$  be a function of  $x$  only (which call  $\psi x$ ), the solution of

$$\frac{d^2 \varpi}{dx^2} + \psi x \frac{d\varpi}{dx} = 0$$

is a solution of the partial, since all the other terms vanish on that assumption. The value of  $\varpi$  thus obtained is  $\int \epsilon^{-\psi_1 x} dx$  ( $\psi_1 x$  being  $\int \psi x dx$ ), and  $\mu$  is then derived by the fundamental system. The constants of these two integrations are immaterial, as they merge in the arbitrary constant. Secondly, if  $\frac{Q}{\phi_2}$  is a function of  $y$  only (which call  $\psi y$ ), we have a similar value of  $\varpi$ , viz.  $\int \epsilon^{-\psi y} dy$ , and corresponding results.

*Ex. 1.* Let  $\phi_1 = \frac{1}{x}$ ,  $\phi_2 = \frac{1}{x\epsilon^y}$ ,  $R^2 = \frac{1}{x^2} - \frac{1}{x\epsilon^y}$ ; then  $P = \frac{1}{x}$ , and  $\varpi = \log x$ . The fundamental system then gives

$$\frac{d\mu}{dy} = \left(1 - \frac{x}{\epsilon^y}\right)^{-\frac{1}{2}}, \quad \frac{d\mu}{dx} = -\frac{1}{x} \left(1 - \frac{x}{\epsilon^y}\right)^{-\frac{1}{2}},$$

whence

$$\mu = \log \left( \frac{2\epsilon^y}{x} - 1 + \sqrt{\frac{4\epsilon^{2y}}{x^2} - \frac{4\epsilon^y}{x}} \right);$$

and the complete primitive is

$$c^2 x^2 - 2cx \left( \frac{2\epsilon^y}{x} - 1 \right) + 1 = 0.$$

*Ex. 2.* Let  $\phi_1 = \frac{y}{x}$ ,  $\phi_2 = 2b\frac{y}{x} - a^2$ ,  $R^2 = \frac{y^2}{x^2} - 2b\frac{y}{x} + a^2$ ; then  $P = \frac{1}{x}$ , and  $\varpi = \log x$ . The fundamental system then gives

$$\frac{d\mu}{dy} = (y^2 - 2bxy + a^2 x^2)^{-\frac{1}{2}}, \quad \frac{d\mu}{dx} = -\frac{1}{x} (y^2 - 2bxy + a^2 x^2)^{-\frac{1}{2}},$$

whence

$$\mu = \log (y - bx + \sqrt{y^2 - 2bxy + a^2 x^2});$$

and the complete primitive is

$$c^2 x^2 - 2cx \left( \frac{y}{x} - b \right) + b^2 - a^2 = 0 \text{ (Boole, p. 185).}$$

*Ex. 3.* Let

$$\phi_1 = \log x, \quad \phi_2 = \log x \left( \frac{y}{x} + 1 \right), \quad R^2 = \log x \left( \log x - \frac{y}{x} - 1 \right);$$

$$\text{then } P = \frac{1 - \log x}{2x}, \text{ and } \varpi = \int \left( \frac{\log x}{x} \right)^{\frac{1}{2}} dx = 2\chi x \text{ suppose.}$$

The system then gives

$$\frac{d\mu}{dy} = (x \log x - y - x)^{-\frac{1}{2}}, \quad \frac{d\mu}{dx} = -\log x (x \log x - y - x)^{-\frac{1}{2}},$$

whence

$$\mu = -2(x \log x - y - x)^{\frac{1}{2}};$$

and the complete primitive is

$$c^2 - 2c\chi x + (\chi x)^2 - x \log x + y + x = 0.$$

*Ex. 4.* Let  $\phi_1 = (1 + x^3)y$ ,  $\phi_2 = y^2(1 - 2x^3 + 4x^2 \log y)$ ; then  $P = -\frac{1}{x}$ , and  $\varpi = \frac{1}{2}x^2$ . The system then gives

$$\mu = -\frac{1}{2}(x^4 + 4x - 4 \log y);$$

and the complete primitive is

$$c^2 - cx^2 + \log y - x = 0 \text{ (Boole, p. 184).}$$

*Ex. 5.* Let  $\phi_1 = \frac{1}{2}xy$ ,  $\phi_2 = y^2 \log y$ ,  $R^2 = \frac{y^2}{4}(x^2 - 4 \log y)$ ; then  $\frac{Q}{\phi_2} = \frac{1 + \log y}{y \log y}$ , and  $\varpi = \log \log y$ . The system gives

$$\frac{d\mu}{dy} = \frac{x}{y \log y} (x^2 - 4 \log y)^{-\frac{1}{2}}, \quad \frac{d\mu}{dx} = -2(x^2 - 4 \log y)^{-\frac{1}{2}},$$

whence

$$\mu = -2 \log \left( \frac{x}{\sqrt{\log y}} + \left( \frac{x^2}{\log y} - 4 \right)^{\frac{1}{2}} \right);$$

and the primitive becomes, after reduction,

$$c^2 - cx + \log y = 0 \text{ (Boole, p. 162).}$$

*Ex. 6.* Let  $\phi_1 = \frac{\epsilon^{2x}}{y}$ ,  $\phi_2 = \epsilon^{2x}$ ,  $R^2 = \epsilon^{4x} \left( \frac{1}{y^2} - \frac{1}{\epsilon^{2x}} \right)$ ; then

$$\frac{Q}{\phi_2} = \frac{1}{y}, \text{ and } \varpi = \log y.$$

The system gives

$$\frac{d\mu}{dy} = \frac{1}{y} \left( 1 - \frac{y^2}{\epsilon^{2x}} \right)^{-\frac{1}{2}}, \quad \frac{d\mu}{dx} = - \left( 1 - \frac{y^2}{\epsilon^{2x}} \right)^{-\frac{1}{2}},$$

whence

$$\mu = \log \left( \frac{\epsilon^x}{y} + \sqrt{\frac{\epsilon^{2x}}{y^2} - 1} \right);$$

and the primitive is

$$c^2 y^2 - 2c\epsilon^x + 1 = 0.$$

*Ex. 7.* Let  $\phi_1 = \frac{2y\epsilon^{2x}}{y^2 + \epsilon^{2x}}$ ,  $\phi_2 = \frac{2y^2\epsilon^{2x}}{y^2 + \epsilon^{2x}}$ ; then

$$\frac{Q}{\phi_2} = \frac{1}{y}, \text{ and } \varpi = \log y.$$

The system gives

$$\frac{d\mu}{dy} = \frac{2^{\frac{1}{2}}}{y} (1 - y^2 \epsilon^{-2x})^{-\frac{1}{2}}, \quad \frac{d\mu}{dx} = -2^{\frac{1}{2}} (1 - y^2 \epsilon^{-2x})^{-\frac{1}{2}},$$



whence

$$\mu = 2^{\frac{1}{2}} \log \left( \frac{\epsilon^x}{y} - \sqrt{\frac{\epsilon^{2x}}{y^2} - 1} \right);$$

and the primitive is

$$(cy^{-2\frac{1}{2}})^2 - 2 \frac{\epsilon^x}{y} (cy^{-2\frac{1}{2}}) + 1 = 0.$$

*Ex. 8.* Let  $\phi_1 = \frac{x}{2y}$ ,  $\phi_2 = \frac{1}{4} \left( \frac{x^2}{y^2} - \frac{x^2}{1+x^2} \right)$ ,  $R^2 = \frac{1}{4} \frac{x^2}{1+x^2}$ ;

then

$$\frac{Q}{\phi_2} = -\frac{2}{y}, \text{ and } \varpi = \frac{1}{3} y^3.$$

The system gives

$$\frac{d\mu}{dy} = y(1+x^2)^{\frac{1}{2}}, \quad \frac{d\mu}{dx} = \frac{1}{2} xy^2(1+x^2)^{-\frac{1}{2}} - \frac{1}{2} x(1+x^2)^{\frac{1}{2}},$$

whence

$$\mu = \frac{y^2}{2} (1+x^2)^{\frac{1}{2}} - \frac{1}{6} (1+x^2)^{\frac{3}{2}};$$

and the primitive is

$$c - \frac{y^3}{3} \pm (1+x^2)^{\frac{1}{2}} \left( \frac{y^2}{2} - \frac{1}{6} (1+x^2) \right) = 0,$$

or

$$c^2 - 2c \frac{y^3}{3} - \left( \frac{(1+x^2)^3}{36} - \frac{1}{9} y^2(1+x^2)^2 + \frac{1}{4} y^4(1+x^2) - \frac{1}{9} y^6 \right) = 0.$$

(Peacock, p. 342).

*Ex. 9.* Let  $\phi_1 = yf_1x + f_2x$ , and  $R = f_3x$ . Then  $P$  is a function of  $x$  only; and  $\varpi$  will be found to be

$$\varpi = \int f_3x e^{-\int f_1x dx} dx.$$

The given equation is, in fact, the product of the two linear equations,

$$p = yf_1x + f_2x \pm f_3x,$$

both of which have the same integrating factor.

13. Other methods of solution are obtainable by determining upon what hypothesis  $\varpi$  or  $\mu$  will have a particular form. If  $t$  be an argument containing  $x$  and  $y$ , we can ascertain at once whether it is possible for a function of  $t$  to satisfy the partial equation; and if it do, we can determine the form of the function. For we have (calling the function  $\chi$ )

$$\begin{aligned} & \chi''t \left( \left( \frac{dt}{dx} \right)^2 + 2\phi_1 \frac{dt}{dx} \frac{dt}{dy} + \phi_2 \left( \frac{dt}{dy} \right)^2 \right) \\ & + \chi't \left( \frac{d^2t}{dx^2} + 2\phi_1 \frac{d^2t}{dx dy} + \phi_2 \frac{d^2t}{dy^2} + P \frac{dt}{dx} + Q \frac{dt}{dy} \right) = 0. \end{aligned}$$

Therefore if the coefficient of  $\chi' t$  divided by that of  $\chi'' t$ , is a function of  $t$  (which call  $\lambda t$ ), then  $t$  is a possible argument; and  $\chi t$  is equivalent to  $\int e^{-\lambda_1 t} dt$ ,  $\lambda_1 t$  being  $\int \lambda t dt$ .

Suppose, for example, we desire to know what must be the constitution of  $\phi_1$  and  $\phi_2$  in order that  $\varpi$  or  $\mu$  may be a function of  $xy$ . It will be seen that they must be such that

$$\frac{2\phi_1 + Py + Qx}{y^2 + 2\phi_1 xy + \phi_2 x^2}$$

is a function of  $xy$ .

*Ex. 10.* Thus, taking  $\phi_1 = a$ ,  $\phi_2 = \frac{y^2}{x^2}$ , we find

$$P = \frac{y}{x^2} \left( a + \frac{y}{x} \right)^{-1}, \quad Q = -\frac{y^2}{x^3} \left( a + \frac{y}{x} \right)^{-1} + \frac{2y}{x^2};$$

and the expression above written becomes  $(xy)^{-1}$ . Consequently  $\mu = \log(xy)$ , from which  $\varpi$  and the primitive can be found.

Again, the condition that one of the two,  $\varpi$  and  $\mu$ , may be a function of  $\frac{y}{x}$  (or  $t$ ) is

$$\frac{2(t - \phi_1) + Qx - Py}{t^2 - 2\phi_1 t + \phi_2} = \text{a function of } t.$$

Thus if  $\phi_1 = a$ ,  $\phi_2 = \frac{y^2}{x^2}$ , this condition is fulfilled; and we find

$$\lambda t = \frac{1}{t} + \frac{a}{t^2 - a^2}.$$

Therefore

$$\varpi = \sin^{-1} \frac{t}{a} - \log \left( \frac{a}{t} + \sqrt{\frac{a^2}{t^2} - 1} \right);$$

and since

$$\mu = \log(xy),$$

the complete primitive is found.

In this example we cannot directly rationalize the primitive into a quadratic in  $c$ ; but if we take the forms

$$cf + \frac{1}{cf} = \frac{2ax}{y},$$

$$cf' + \frac{1}{cf'} = 2 \frac{ax}{y},$$

$f$  being  $xye^{-\sin^{-1} \frac{y}{ax}}$ , and  $f'$  being  $xye^{\sin^{-1} \frac{y}{ax}}$ , we have by multi-

plication a biquadratic in  $c$ , which includes also the solution of

$$p^2 + 2ap + \frac{y^2}{x^2} = 0,$$

being in fact the primitive of

$$p^4 - 2\left(2a^2 - \frac{y^2}{x^2}\right)p^2 + \frac{y^4}{x^4} = 0.$$

If  $\phi_1$  and  $\phi_2$  are both functions of  $\frac{y}{x}$  or  $t$ ,  $R$  is so also;  $Qx$  and  $Py$ , too, are functions of  $t$ ; so that the condition is satisfied. This gives the solution of homogeneous equations of the second degree.

*Ex. 11.* Let  $\phi_1 = \frac{y}{x}$ ,  $\phi_2 = \frac{1}{x} \frac{y^2}{x^2} - (n-1)$ ,  $R^2 = \frac{n-1}{n} \left( \frac{y^2}{x^2} + n \right)$ ,  
whence

$$P = \frac{1}{x}, \quad Q = \frac{y}{nx^2}; \quad Qx - Py = t \left( \frac{1}{n} - 1 \right); \quad \lambda t = \frac{t}{t^2 + n};$$

$$\lambda_1 t = \frac{1}{2} \log(t^2 + n); \quad \chi t = \int \frac{dt}{(t^2 + n)^{\frac{1}{2}}} = \log(t + \sqrt{t^2 + n}).$$

This being  $\varpi$ ,  $\mu$  is found to be  $\left( \frac{n-1}{n} \right)^{\frac{1}{2}} \log x$ ; and the primitive is

$$cf + \frac{1}{cf} = 2 \frac{y}{x}, \quad f \text{ being } x \left( \frac{n-1}{n} \right)^{\frac{1}{2}} \quad (\text{Boole, p. 130}).$$

We may go on thus indefinitely deducing special theorems for any function of  $x$  and  $y$ . Thus  $\mu$  will be a function of  $x + ay$  if

$$\frac{P + aQ}{1 + 2a\phi_1 + a^2} \text{ be a function of } (x + ay).$$

The process, however, is tentative; for the forms of  $\phi_1$  and  $\phi_2$  do not generally supply any indication as to what particular arguments can be employed with a probability of success.

14. Reverting to the primitive in the form

$$(cf + \chi)^2 - 2\phi_1(cf + \chi) + \phi_2 = 0,$$

we may observe that there is a class of equations which admit of  $\chi$  being zero—that is, having a primitive of the form

$$(cf)^2 - 2\phi_1(cf) + \phi_2 = 0.$$

Clairaut's equation, when of the second degree, is an instance of this, and in it  $f$  is unity.

If we place the equation in the form

$$(cf)^2 - 2v(cf) + 1 = 0, \quad v \text{ being } \frac{\phi_1}{\sqrt{\phi_2}},$$

and differentiate, divide by  $2cf$ , and expel  $c$  by observing that  $cf = \frac{p}{\sqrt{\phi_2}}$ , and therefore  $cf' = \frac{p}{\sqrt{\phi_2}} \mu'$ , and compare the differential equation thus arrived at with our given form, we shall ultimately obtain the system

$$\begin{aligned}\frac{d\mu}{dy} &= \frac{1}{R} \left( \frac{d\varpi}{dx} + \phi_1 \frac{d\varpi}{dy} \right), \\ \frac{d\mu}{dx} &= -\frac{1}{R} \left( \phi_1 \frac{d\varpi}{dx} + \phi_2 \frac{d\varpi}{dy} \right); \end{aligned}$$

where  $\varpi$  is not now unknown, but is equal to  $\log(v + \sqrt{v^2 - 1})$ . The partial equation therefore becomes an equation of condition, denoting that the solution is of the form suggested; and the system last above written determines  $\mu$  or  $f$ . Example 5 above is an instance of this; and  $\mu$  will be found to be  $-\frac{1}{2} \log \log y$ , and  $f$  is  $(\log y)^{-\frac{1}{2}}$ . The reader may ascertain that the method is applicable to

*Ex. 12.*  $p^2 + \left( a \frac{x}{y} + b \frac{y}{x} + \frac{c}{xy} \right) p + e = 0$  (Boole, p. 135), and find  $\mu$  and  $f$ .

Equations of this kind are not uncommon, a circumstance which renders it often worth while to try if  $\frac{\phi_1}{\sqrt{\phi_2}}$  be a possible argument for the solution of the partial equation.

15. Another particular case deserving of special notice is that in which  $\phi_2 = 1$ , each value of  $p$  being the reciprocal of the other. If, moreover,  $\phi_1$  be symmetrical with regard to  $x$  and  $y$ , the partial equation will be so also, a circumstance likely to suggest a solution of it. When  $\phi_2 = 1$ ,  $P$  and  $Q$  are most easily found from

$$\begin{aligned}P + Q &= - \left( \frac{d}{dy} + \frac{d}{dx} \right) \log(\phi_1 - 1), \\ P - Q &= \left( \frac{d}{dy} - \frac{d}{dx} \right) \log(\phi_1 + 1). \end{aligned}$$

Take, for example, the equation

$$p^2 - \frac{y^2 + x^2 + a^2 - 4bxy}{xy - b(x^2 + y^2 + a^2)} p + 1 = 0;$$

we find

$$\begin{aligned}P + Q &= (2b - 1) \frac{x + y}{xy - b(x^2 + y^2 + a^2)}, \\ P - Q &= (2b + 1) \frac{y - x}{xy - b(x^2 + y^2 + a^2)}.\end{aligned}$$



It is not difficult to see that  $\mu$  and  $\varpi$  are respectively functions of  $x+y$  and  $x-y$ , for we have

$$\frac{P+Q}{2(1+\phi_1)} = \frac{x+y}{(x+y)^2+a^2}, \quad \frac{P-Q}{2(1-\phi_1)} = \frac{x-y}{(x-y)^2+a^2};$$

and in fact we find

$$\mu = \sqrt{1+2b} \log (x+y + \sqrt{(x+y)^2+a^2}), \\ \varpi = \sqrt{1-2b} \log (x-y + \sqrt{(x-y)^2+a^2});$$

which gives the solution in the form

$$(C - (x+y + \sqrt{(x+y)^2+a^2})^{\sqrt{1+2b}} (x-y + \sqrt{(x-y)^2+a^2})^{\sqrt{1-2b}}) \\ (C - (x+y + \sqrt{(x+y)^2+a^2})^{\sqrt{1+2b}} (x-y + \sqrt{(x-y)^2+a^2})^{-\sqrt{1-2b}}) = 0.$$

If in this differential equation we make  $x = x'\sqrt{-1}$ , we have the equation of the trajectory of a system of confocal ellipses discussed by Mr. Boole at p. 246. The reader may compare the modes of solution, and the forms of the result.

16. Equations of the second degree which admit of being placed in the form  $y = xfp + \phi p$  need not, of course, be integrated by the formulæ which I have given, as they are otherwise soluble. The existing mode of solution, however, enables us in these cases to determine the forms of  $\mu$  and  $\varpi$ ; and the use of this process exhibits the complete primitive in some cases in a more convenient form than the ordinary solution. Consider the example

$$p^2 - py - x = 0 \quad (\text{Boole, p. 127}),$$

in which  $fp = -\frac{1}{p}$ , and  $\phi p = p$ . It is easy to find that

$$Fp = \left(1 + \frac{1}{p^2}\right)^{\frac{1}{2}}, \text{ and } \Phi p = \log (p - \sqrt{p^2-1});$$

and  $p$  has the two values  $\frac{1}{2}(y \pm \sqrt{y^2+4x})$ . With these we have to form

$$(c - (xFp_1 + \Phi p_1))(c - (xFp_2 + \Phi p_2)) = 0;$$

but as the functions of  $p_1$  and  $p_2$  are transcendental, we cannot express finitely their symmetrical compounds.

Making use of this solution so far as it goes, we find (to adopt the method of this paper)

$$\frac{d\mu}{dx} \div \frac{d\mu}{dy} = \frac{1+x}{y} + \sqrt{\left(\frac{1+x}{y}\right)^2 + 1}, \\ \frac{d\varpi}{dx} \div \frac{d\varpi}{dy} = \frac{1+x}{y} - \sqrt{\left(\frac{1+x}{y}\right)^2 + 1}.$$

Consequently  $\mu$  and  $\varpi$  are respectively functions of

$$(y^2 + (1+x)^2)^{\frac{1}{2}} \pm x,$$

or of  $t_1$  and  $t_2$  suppose. In fact we have

$$\mu = \int \epsilon^{\int \frac{dt_1}{t_1^2-1}} dt_1 = \int \left( \frac{t_1+1}{t_1-1} \right)^{\frac{1}{2}} dt = (t_1^2-1)^{\frac{1}{2}} + \log(t_1 + \sqrt{t_1^2-1}),$$

$\varpi$  = same function of  $t_2$ .

The primitive may be placed in the form

$$(c - \epsilon^{\varpi+\mu})(c - \epsilon^{\varpi-\mu}) = 0,$$

which, with the above values of  $\varpi$  and  $\mu$ , probably admits of being rationalized. At all events we learn that the only functions which enter in the complete primitive are

$$x \pm (y^2 + (1+x)^2)^{\frac{1}{2}},$$

a circumstance which it would be difficult to infer from the ordinary mode of solution.

The method has now been sufficiently illustrated in reference to equations of the second degree. I reserve the application of the principles of this paper to equations of the third and higher degrees, until I have had further opportunities of studying them.

### *Discussion of the Partial Equation.*

17. The partial differential equation on which we depend for the integration of equations of the second degree,

$$\text{viz. } r + 2\phi s + \phi_2 t + Pp + Qq = 0,$$

is worthy of independent consideration.

It will be found to be soluble by Monge's method. His auxiliary equations are

$$\left( \frac{dy}{dx} \right)^2 - 2\phi_1 \frac{dy}{dx} + \phi_2 = 0,$$

$$dp dy + \phi_2 dy dx + (Pp + Qq) dy dx = 0;$$

the first of which is the original differential equation of the above paper, for this purpose considered as integrable. This equation is resolvable into

$$dy = (\phi_1 \pm R) dx;$$

one of which reduces the second equation to

$$D \log \frac{1}{R} (p + (\phi_1 - R)q) + \frac{d}{dy} \log (\phi_1 + R) dy = 0,$$

in which  $D$  is used to denote the complete differential of the function subjected to it. Multiplying this by  $\lambda$ , an assumed

function of  $x$  and  $y$ , we have

$$\begin{aligned} D \log \left( \frac{\lambda}{R} (p + (\phi_1 - R)q) \right) - \left( \frac{d(\log \lambda)}{dx} dx + \frac{d(\log \lambda)}{dy} dy \right) \\ + \frac{d}{dy} \log (\phi_1 + R) dy = 0, \end{aligned}$$

which becomes

$$D \log \left( \frac{\lambda}{R} (p + (\phi_1 - R)q) \right) = 0,$$

if  $\lambda$  be so taken as to make the other two terms vanish. Now, if  $c = \tau_1$  be one solution of the first auxiliary equation, this condition is satisfied by making  $\lambda = \left( \frac{d\tau_1}{dy} \right)^{-1}$ , or  $\log \lambda = -\log \frac{d\tau_1}{dy}$ ; for these two terms then become (if multiplied by  $\phi_1 + R$ )

$$(\phi_1 + R) \left( \frac{d^2 \tau_1}{dx dy} dx + \frac{d^2 \tau_1}{dy^2} dy \right) + \frac{d\tau_1}{dy} \frac{d(\phi_1 + R)}{dy} dy;$$

which, being

$$(\phi_1 + R) \frac{d}{dy} \left( \frac{d\tau_1}{dx} + (\phi_1 + R) \frac{d\tau_1}{dy} \right),$$

vanishes identically if  $\tau_1$  be a solution of

$$dy = (\phi_1 + R) dx.$$

Monge's second equation has therefore the two solutions,

$$\left( R \frac{d\tau_1}{dy} \right)^{-1} (p + q(\phi_1 - R)) = \text{constant},$$

$$\left( R \frac{d\tau_2}{dy} \right)^{-1} (p + q(\phi_1 + R)) = \text{constant};$$

$\tau_2$  being the other integral of the first auxiliary. The two first integrals of the partial equation therefore are

$$p + (\phi_1 - R)q = 2R \frac{d\tau_1}{dy} \chi_1 \tau_1,$$

$$p + (\phi_1 + R)q = 2R \frac{d\tau_2}{dy} \chi_2 \tau_2.$$

These give

$$q = \frac{d\tau_2}{dy} \chi_2 \tau_2 - \frac{d\tau_1}{dy} \chi_1 \tau_1,$$

$$p = -(\phi_1 - R) \frac{d\tau_2}{dy} \chi_2 \tau_2 + (\phi_1 + R) \frac{d\tau_1}{dy} \chi_1 \tau_1,$$

whence

$$dz = p dx + q dy = \chi_2 \tau_2 d\tau_2 - \chi_1 \tau_1 d\tau_1,$$

and

$$z = f_1 \tau_1 + f_2 \tau_2.$$

18. By separation of symbols, the partial equation may be written in the form

$$\left( \frac{d}{dx} + (\phi_1 + R) \frac{d}{dy} + P \right) \left( \frac{d}{dx} + (\phi_1 - R) \frac{d}{dy} \right) z = 0;$$

or in the same form with the sign of  $R$  changed. As the solution of this is

$$z = f_1 \tau_1 + f_2 \tau_2,$$

which gives

$$\frac{dz}{dx} + (\phi_1 - R) \frac{dz}{dy} = 2R \frac{d}{dy} f_1 \tau_1,$$

we observe that the solution of any equation of the form

$$\frac{dv}{dx} + (\phi_1 + R) \frac{dv}{dy} + Pv = 0$$

is

$$v = 2R \frac{d}{dy} (f_1 \tau),$$

where  $c = \tau$  is the solution of  $\frac{dy}{dx} = \phi_1 + R$ .

The partial equation admits of some transformations; among which I may mention, that if we add to it an absolute term

$$R \left( \frac{d}{dx} \left( \frac{1}{R} \frac{d\phi_1}{dy} \right) + \frac{1}{2} \frac{d}{dy} \left( \frac{1}{R} \frac{d\phi_2}{dy} \right) \right),$$

the effect is to add to the value of  $z$

$$\frac{1}{2} \log \left( \left( \frac{df_1}{dx} \right)^2 + 2\phi_1 \frac{df_1}{dx} \frac{df_1}{dy} + \phi_2 \left( \frac{df_1}{dy} \right)^2 \right) - \log R,$$

with a similar function of  $f_2$ .

If we add to the partial the absolute term

$$R \left( \frac{d}{dx} \left( R \frac{d\frac{1}{R}}{dy} \right) + \frac{d}{dy} \left( R \frac{d\frac{\phi_1}{R}}{dy} \right) \right),$$

the value of  $z$  is augmented by two functions of the form

$$\frac{1}{2} \log \left( \frac{df_1}{dx} + (\phi_1 + R) \frac{df_1}{dy} \right) - \frac{1}{2} \log \left( \frac{df_1}{dx} + (\phi_1 - R) \frac{df_1}{dy} \right).$$



*Note on the application to ordinary Equations of the principle of Duality established for partial Equations.*

19. For an exposition of this principle see Boole's *Diff. Eq.* p. 366. Applied to ordinary equations of the first order, the principle gives this proposition.

If we have two ordinary differential equations,

$$\phi(x, y, p) = 0, \quad \phi(p, px - y, x) = 0,$$

and either of them is solved by

$$y = \psi x,$$

the other is solved by

$$y = x\psi'^{-1}x - \psi\psi'^{-1}x.$$

This principle has its most extensive application to the equation so often above referred to,

$$y = xfp + \phi p.$$

Its dual is

$$px - y = pfx + \phi x,$$

or

$$p = \frac{y}{x - fx} + \frac{\phi x}{x - fx},$$

whose solution is

$$y = \epsilon \int \frac{dx}{x - fx} \left( c + \int \frac{\phi x}{x - fx} \epsilon^{-\int \frac{dx}{x - fx}} dx \right) = \lambda x \text{ (suppose).}$$

Consequently

$$y = x\lambda'^{-1}x - \lambda\lambda'^{-1}x$$

is the solution of the given equation. This form, however, often involves the inversion of transcendental functions. If we take  $y = \lambda x$ , its dual is

$$y = px - \lambda p, \text{ or } y - px + \lambda p = 0,$$

in which we have merely to substitute for  $p$  its  $n$  values successively, and multiply together the  $n$  resulting expressions.

If  $fp = p$  (Clairaut's form), the mere process of transformation gives an integral. For

$$y - px = \phi p$$

transforms itself into

$$y = \phi x,$$

whence the solution

$$y = x\phi'^{-1}x - \phi\phi'^{-1}x.$$

As no arbitrary constant is thus introduced, it is obvious that in this case the solution arrived at must be the singular solution.

The transformation is applicable to all equations having algebraic coefficients.

It is also applicable to explicit integration, and teaches us that if any function of  $x$  is integrable with regard to  $x$ , its inverse is not only integrable, but its integral is known without any new process of integration. This is implied in the identity

$$\int \psi^{-1} x dx = x \psi^{-1} x - \psi_1 \psi^{-1} x; \quad \psi_1 t \text{ being } \int \psi t dt.$$

Thus, if

$$\int \cos x dx = \sin x + c, \quad \int \cos^{-1} x dx = x \cos^{-1} x - \sin \cos^{-1} x + c.$$

If

$$\int \epsilon^x dx = \epsilon^x, \quad \int \log x dx = x \log x - \epsilon^{\log x} + c.$$

If

$$\int \tan x dx = c - \log \cos x, \quad \int \tan^{-1} x dx = x \tan^{-1} x + c - \log \cos (\tan^{-1} x);$$

and so on.

Even if the inverted function be not expressible, yet we can express its relation with its integral. Thus if  $\lambda x$  denote the inverse of  $x\epsilon^x$ , since  $\int x\epsilon^x = x\epsilon^x - \epsilon^x$ , we have

$$\int \lambda x dx = x \lambda x + \epsilon^{\lambda x} - \lambda x \epsilon^{\lambda x} + c.$$

If  $\lambda x$  denote the inverse of  $ax^n + x$ , then (using an obvious reduction)

$$\int \lambda x dx = \frac{n}{n+1} x \lambda x - \frac{1}{2} \frac{n-1}{n+1} (\lambda x)^2 + c.$$

I do not remember seeing this method given in elementary works as a process of integration.

Fitzwilliam Square, Dublin,  
April 5, 1864.

## LIX. *Note on the Constitution of the Sun.*

By G. MAGNUS\*.

SO long ago as the year 1795, W. Herschel put forward the view that the sun consists of a dark nucleus which is surrounded with a photosphere, or atmosphere giving out light and heat†. Between the latter and the nucleus he assumed the existence of a light-reflecting atmosphere, which, in virtue of this property of reflecting, hindered the illumination of the nucleus by the photosphere. In treating of this hypothesis‡, which he designated as the one generally adopted§, Arago made

\* Translated by Prof. Wanklyn from Poggendorff's *Annalen*, No. 3, 1864.

† Phil. Trans. 1795, p. 46.

‡ *Astronomie*, vol. ii. p. 94 (Arago).

§ Ibid. p. 143.

the remark that the photosphere defined the outermost border of the sun, but that the sun, as he inferred from observations on the protuberances seen during a total eclipse, is surrounded by a transparent atmosphere. Herschel said that the photosphere was neither a liquid nor an elastic fluid, but that it consisted of luminous clouds\*. According to our present knowledge of the radiation of light and heat, it is difficult to suppose that the photosphere, which sends out the heat of the sun, should not have the nucleus which it encloses heated to ignition. The reflecting atmosphere assumed to exist between it and the nucleus may indeed hinder the illumination of the latter, but not its becoming gradually heated. Kirchhoff† is therefore right when he says that this hypothesis for the explanation of the spots on the sun is so completely in opposition to the higher physical principles that we must abandon it, even if we are not in a position to offer any other explanation which would render the spots on the sun at all intelligible. His investigations of the solar spectrum have led Kirchhoff to the supposition that the sun is composed of a solid or liquid nucleus which is at the highest temperature of ignition, and is surrounded by a transparent atmosphere of somewhat lower temperature.

Up to the present time no one, so far as I know, has drawn any conclusion relative to the constitution of the sun from the heat which it gives out; the observations of Secchi‡ upon the smaller quantity of heat which is radiated from the poles than from the equator of the sun may, however, be regarded as an example in point. Some experiments which I have made upon the radiation of heat give, I believe, a new insight into the constitution of this heavenly body. If we observe the heat radiating from a non-luminous gas-light and then introduce some soda into the flame, whereby, as is well known, it will become very luminous, we shall find that the radiation of heat will also increase. The disposition of the experiment was so that a fixed spot in the soda-flame was always compared with the same spot in the non-luminous flame, and that, moreover, the soda introduced into the flame could not radiate against the thermo-pile which served for the observation. Although obviously a part of the heat of the flame was taken up in heating to redness the soda and the platinum wire which carried the soda, and in converting the soda into vapour—although therefore the flame had altogether a lower temperature than before when it was non-luminous, yet nevertheless it radiated about a third more heat than before.

\* Phil. Trans. 1795, p. 71.

† *Denkschriften der Berliner Acad. der Wiss.* 1861, p. 85.

‡ *Comptes Rendus*, vol. xxxv. p. 606; and vol. xxxvi. p. 659.

Whether the soda exists in the luminous flame in the form of vapour, or whether single torn off pieces of this body increase the illuminating power, will not here be considered; for the sake of brevity I will choose the designation soda-vapour. When, instead of this vapour, a piece of platinum was brought into the part of the flame which was experimented upon, a still greater radiation of heat occurred. Obviously the platinum plate abstracted more heat than the soda, and yet the radiation was greater. By taking a plate 55 millims. in diameter, the radiation from the flame amounted to nearly double as much as previously, when the flame was non-luminous. Whether a platinum plate somewhat thicker or thinner was used made no apparent difference, provided that it had always the same diameter. But if, instead of making the plate thicker, it was covered with carbonate of soda, then the radiation would increase afresh, and so considerably as to become half as much greater as before, when the plate without sodium was used.

The radiation rose still higher when, in addition to platinum covered with soda, there was also present soda-vapour in the flame, arising from soda on a platinum wire placed deep in the flame where the soda could not directly radiate against the pile, as was above described.

In the flame altogether saturated with soda-vapour, the platinum covered with soda radiated nearly three times as much heat as the non-luminous flame.

Lithium and strontium-salts acted like sodium-salts.

These experiments show that gaseous bodies radiate very much less heat than solids or liquids; it can therefore hardly be maintained that a gaseous or vaporous (*dampförmige*) photosphere is the seat of the solar heat. They show, further (and it is very remarkable), that ignited soda has much more radiating power than platinum heated to an equal temperature.

Furthermore, they show that the soda-vapour or the fine particles of soda absorb only a little of the heat which is evolved by the solid or liquid body; for the radiation of the solid body in the flame charged with soda-vapour was indeed always less than the sum of the radiation of the solid body alone and the vapour alone, but it was only a little less than this sum.

This behaviour of ignited soda in the liquid and gaseous form supports Kirchhoff's theory of the constitution of the sun in a striking manner.



LX. *On the Duration of the Combustion of Fuses under different Atmospheric Pressures.* By Captain J. MITCHELL.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

Madras, March 11, 1864.

I HAVE only lately seen a paper which appeared at p. 156 of No. 166 of this Journal. This paper contains one or two errors which M. Dufour, I have no doubt, will be glad to have corrected.

The contribution to which he refers was made by Troop Quarter-Master J. Mitchell, of the Madras Horse Artillery; and the fuses were not burned at the Himalayas, but at the Artillery Depot at St. Thomas's Mount near Madras, at Bungalore on the tableland of Mysore, and at Kotagherry and Ootacamund on the Neilgherries, at the respective altitudes of about 180, 3000, 6500, and 7300 feet above the level of the sea.

The experiment made here was on too limited a scale to do more than show that the combustion of fuses is retarded at considerable elevations; but that was all that was sought. Our fuses had burned too long at the annual practice at Bungalore on two following years, and I pointed out the difference of altitude between Bungalore and St. Thomas's Mount as the probable cause.

It has given me great pleasure to see that M. Dufour's experiments, which seem to have been conducted with very great care, have proved what mine only indicated, viz. that the increased time of burning is in a constant ratio to the increased altitude of the place, which is a fact that should be remembered by artillerymen when employed in elevated regions.

M. Dufour considers diminished pressure the *sole* cause of the retardation of combustion, or, in other words, of the increased time a certain length of fuse will burn at considerable altitudes. It is quite true, as remarked by him, that fuse composition is rich in oxygen; but it is *also true that the amount of oxygen in the fuse is constant*, while that in the atmosphere becomes less and less as we leave the level of the sea; and it does therefore seem probable that a diminished supply of oxygen, if not the sole, is at least partly the cause of combustion becoming less active as we ascend above the level of the sea. His plan of burning fuses in carbonic acid gas appears to prove the contrary; whether it does not prove too much is a question I can only put, as I have no means of conducting the experiments that would enable me to confirm, or otherwise, those of M. Dufour.

I am, Gentlemen,

Your most obedient Servant,

J. MITCHELL, *Captain,*  
*Superintendent Madras Museum.*

LXI. *On the Cause of the Cooling Effect produced on Solids by Tension.* By Mr. JAMES CROLL\*.

FROM a series of experiments made by Dr. Joule with his usual accuracy, he found that when bodies are subjected to tension, a cooling effect takes place. "The quantity of cold," he says, "produced by the application of tension was sensibly equal to the heat evolved by its removal; and further, that the thermal effects were proportional to the weight employed"†. He found that when a weight was applied to compress a body, a certain amount of heat was evolved; but the same weight, if applied to stretch the body, produced a corresponding amount of cold.

This, although it does not appear to have been remarked, is a most singular result. If we employ a force to compress a body, and then ask what has become of the force applied, it is quite a satisfactory answer to be told that the force is converted into heat, and reappears in the molecules of the body as such; but if the same force be employed to stretch the body, it will be no answer to be told that the force is converted into cold. Cold cannot be the force under another form, for cold is a privation of force. If a body, for example, is compressed by a weight, the *vis viva* of the descending weight is transmitted to the molecules of the body and reappears under that form of force called heat; but if the same weight is applied so as to stretch or expand the body, not only does the force of the weight disappear without producing heat, but the molecules which receive the force lose part of that which they already possessed. Not only does the force of the weight disappear, but along with it a portion of the force previously existing in the molecules under the form of heat. We have therefore to inquire, not merely into what becomes of the force imparted by the weight, but also what becomes of the force in the form of heat which disappears from the molecules of the body itself. That the *vis viva* of the descending weight should disappear without increasing the heat of the molecules is not so surprising, because it may be transformed into some other form of force different from that of heat. For it is by no means evident *à priori* that heat should be the only form under which it may exist. But it is somewhat strange that it should cause the force previously existing in the molecules in the form of heat also to change into some other form.

When a weight, for example, is employed to stretch a solid body, it is evident that the force exerted by the weight is consumed in work against the cohesion of the particles, for the entire

\* Communicated by the Author.

† Phil. Trans. for 1859, p. 91.

force is exerted so as to pull them separate from each other. But the cooling effect which takes place shows that more force disappears than simply what is exerted by the weight; for the cooling effect is caused by the disappearance of force in the shape of heat from the body itself. The force exerted by the weight disappears in performing work against the cohesion of the particles of the body stretched. But what becomes of the force in the form of heat which disappears from the body at the same time? It must be consumed in performing work of some kind or other. The force exerted by the weight cannot be the cause of the cooling effect. The transference of force from the weight to the body may be the cause of a heating effect—an increase of force in the body; but this transference of force to the body cannot be the cause of a decrease of force in the body. If a decrease of force actually follows the application of tension, the weight can only be the occasion, not the cause of the decrease.

In what manner, then, does the stretching of the body by the weight become the occasion of its losing force in the shape of heat? Or, in other words, what is the cause of the cooling effects which result from tension? The probable explanation of the phenomenon seems to be this: if the molecules of a body are held together by any force, of whatever nature it may be, which prevents any further separation taking place, then the entire heat applied to such a body will appear as temperature; but if this binding force becomes lessened so as to allow further expansion, then a portion of the heat applied will be lost in producing expansion. All solids at any given temperature expand until the expansive force of their heat exactly balances the cohesive force of their molecules, after which no further expansion at the same temperature can possibly take place while the cohesive force of the molecules remains unchanged. But if, by some means or other, the cohesive force of the molecules become reduced, then instantly the body will expand under the heat which it possesses, and of course a portion of the heat will be consumed in expansion, and a cooling effect will result. Now tension, although it does not actually lessen the cohesive force of the molecules of the stretched body, yet produces, by counteracting this force, the same effect; for it allows the molecules an opportunity of performing work of expansion, and a cooling effect is the consequence. If the piston of a steam-engine, for example, be loaded to such an extent that the steam is unable to move it, the steam in the interior of the cylinder will not lose any of its heat; but if the piston be raised by some external force, the molecules of the steam will assist this force, and consequently will suffer loss of heat in proportion to the amount of work which they perform. The very same occurs when tension is applied to a solid. Pre-

vious to the application of tension, the heat existing in the molecules is unable to produce any expansion against the force of cohesion. But when the influence of cohesion is partly counteracted by the tension applied, the heat then becomes enabled to perform work of expansion, and a cooling effect is the result.

## LXII. *Proceedings of Learned Societies.*

### ROYAL SOCIETY.

[Continued from p. 236.]

April 23, 1863.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

“On the Distillation of Mixtures: a Contribution to the Theory of Fractional Distillation.” By J. A. Wanklyn, Esq.

There are many points in the boiling of mixtures which are obscure. The tension of the vapours at the temperature whereat the mixture boils, and the proportions in which the constituents of the mixture are present, are not the only factors which determine the relative rates at which the constituents distil. There have, for instance, to be taken into account the adhesion of the liquids to one another, and the vapour-densities of these liquids. On the present occasion I have to call attention to the influence of this latter element, which influence seems to have been lost sight of by most of those who have applied themselves to this subject.

Leaving out of account for a moment the influence of adhesion, and simplifying the influence of the proportion in which the ingredients are present by taking equal weights of two liquids of different boiling-points, we may set down the rates at which these ingredients will distil as determined by the tensions of the liquids and the densities of the vapours. In the first instant of time the quantity of each ingredient which distils will be found by multiplying its tension at the boiling-point of the mixture by its vapour-density. It thus appears that the liquid with the highest tension will not of necessity distil the quickest, for what the other liquids want in tension they may make up by the greater density of the vapours which they give off. And so when we mix a more volatile with a less volatile liquid and proceed to distil the mixture, we shall now and then find that the less volatile liquid distils faster than the more volatile one. I will here bring forward an experiment to illustrate this point.

Vapour-density.

Methyl-alcohol boils at 66° C. .... 1·107

Iodide of ethyle boils at 72° C. .... 5·397

I took 18 grammes of methyl-alcohol and 17 grammes of iodide of ethyle, mixed them, and distilled off rather more than one-third of the mixture. The distillate consisted of

6·0 grammes methyl-alcohol,
8·7 grammes iodide of ethyle,
14·7



which shows that in this case the less volatile constituent had boiled the faster, the less volatile iodide of ethyle having a very much higher vapour-density than methyl-alcohol.

It will be obvious that when the vapour-densities and tensions are inversely proportional, the mixture must distil over unchanged. This influence of vapour-density goes a great way to explain why homologous bodies are so difficult of separation by means of fractional distillation. The more complex the formula the higher the boiling-point, but also the higher the vapour-density, and therefore the greater the value of the vapour. Why oils, &c. distil so readily in steam is also explained; for aqueous vapour is one of the lightest, while oily vapours are generally heavy.

May 7.—Major-General Sabine, President, in the Chair.

The following communication was read:—

“Notes, principally on Thermo-electric Currents of the Ritterian Species.” By C. K. Akin, Esq.

The electromotive force of a thermo-electric couple is a function of the nature of the metals of which it is composed, and of the temperatures of the junctions. It is expressed in this paper by

$$[x, y]_t^T,$$

where  $x$  and  $y$  are names of metals, and  $T$  and  $t$  are temperatures. In this notation Becquerel's two laws become

$$[a, b]_t'' = [a, b]_t'' - [a, b]_t''; \quad . . . . \quad (I.)$$

and

$$(a, c)_t^T = [a, b]_t^T + [b, c]_t^T. \quad . . . . \quad (II.)$$

From (I.) we learn that the electromotive force of a couple may be expressed as the difference of two quantities which are functions of the temperature and of the nature of the circuit, or

$$[x, y]_t^T = [x, y]_T - [x, y]_t. \quad . . . . \quad (III.)$$

From (II.) we learn that any number of metals with their ends at the same temperature may be introduced without effect, or

$$[a, b]_t + [b, c]_t = [a, c]_t. \quad . . . . \quad (IV.)$$

This equation will always be true if

$$[x, y]_t = [x]_t - [y]_t. \quad . . . . \quad (V.)$$

whence we may write (III.)

$$[x, y]_t^T = [x]_t - [y]_t - [x]_T + [y]_T;$$

or, in other words, the electromotive force of a couple may be considered as the difference of the electromotive force of two metals, each of which is found by subtracting its tension at the higher temperature from that of the lower one.

Everything therefore depends on a knowledge of the value of what may be called the electric tension of each metal at the various temperatures. This for every metal is a function of temperature, and may be called, in the language of the paper, a function of the nature (or name) of the metal and the temperature.

(The nature of the metal may be altered otherwise than chemically.)

If the temperature of the metal vary in any way throughout its length, then if it be homogeneous, the electromotive force will depend only on the temperatures of its extremities.

In a circuit of one metal, the author considers that at the junction of the ends there may be a real discontinuity of temperature while there is a continuity of electric current. He regards the explanation of the effect by the stratum of air between the unequally heated ends to be unsatisfactory. Mercury, as is known, will not produce thermo-currents in this way. The author considers that the texture, &c., as well as the chemical nature of the substance, influences the value of the thermo-electric function. He also shows the possibility of the thermo-electric inversions first discovered by Professor Cumming.

May 21.—Major-General Sabine, President, in the Chair.

The following communication was read :—

“On the Nature of the Sun’s Magnetic Action upon the Earth.”  
By Charles Chambers, Esq.

If the sun were a magnet of sufficient power to exert a sensible attraction upon a small magnet at the distance of the earth, it would have a real influence on the earth by inducing magnetism in its soft iron, and an apparent one due to the direct action of the sun upon the magnets used for measuring the earth’s variations of force. As the earth rotates upon its axis, producing a varying relation, as to position, of the place of observation with respect to the sun, a diurnal variation will thus be produced in the forces which act upon the magnetometers, which variation is shown to follow the simple law  $x = A \sin(h + \alpha)$ ,  $x$  being the deviation of the magnet from its normal position,  $h$  the hour-angle of the sun (and for a single day),  $A$  a constant coefficient, and  $\alpha$  a constant angle. A comparison of this result with the laws of the observed diurnal variations shows that direct and inducing action of the sun is not the sole cause of the variations.

An endeavour is then made to prove that if any part of the observed diurnal variations is due to this cause, it is small in comparison with that produced by other forces in operation. This is done by separating from the observed variations the part of them which obeys the law  $x = B \sin(h + \beta)$ , and comparing the variations in the values of  $B$  and  $\beta$  from month to month with those of  $A$  and  $\alpha$ , when it is seen that the former obey a law which has but little similarity to the law of variation of the latter.

June 18.—Major-General Sabine, President, in the Chair.

The following communication was read :—

“On the Measurement of the Chemical Brightness of various portions of the Sun’s Disc.” By Henry Enfield Roscoe, B.A., F.R.S.

The author has applied the method of measurement of the chemical action of sunlight, which Professor Bunsen and he described in a

memoir presented to the Royal Society in November last\*, to the measurement of the chemical brightness of various portions of the solar disc; and although the observations which have as yet been made are only preliminary, yet he thinks that the results obtained are of sufficient interest to warrant his bringing them before the Society.

Secchi has shown† that the calorific radiation of the centre of the sun's disc is nearly double that from its borders, and that the equatorial regions are somewhat hotter than the polar, whilst observers have long noticed a great difference in luminosity between the centre and edge of the disc.

For the purpose of obtaining a measurement of the relative chemical brightness of various portions of the solar disc, the image of the sun, of about 4 inches in diameter, obtained by a 3½-inch refractor‡, was allowed to fall into a camera placed on the instrument, upon a sheet of standard photographic paper prepared according to the method described in the above-mentioned research. The peculiar property of this standard paper is that it can always be prepared of one and the same degree of sensitiveness, and is perfectly homogeneous. The exposure lasted for from 30 to 120 seconds, the sun's motion being carefully followed by a tangent-screw. After exposure, the shade of tint at several points on the picture was determined by comparison with a graduated photographic strip isolated in the pendulum-photometer, and the chemical intensities corresponding to these shades obtained by reference to the Table given in the memoir above cited. The following numbers give the chemical brightness, thus obtained, at various points on the sun's disc on May 9th, 1863. From these numbers it is seen that the intensity of the chemically active rays at the centre is from three to five times as great as that at the edge of the disc, the chemical rays thus showing a wider variation than the calorific rays exhibited as determined by Secchi. This is doubtless owing to the relatively greater absorption effected by the solar atmosphere on the more refrangible chemical rays.

**Chemical Brightness of Sun's Disc on May 9, 1863.**

1. At centre of Sun's Disc.		2. At 15° from edge of Sun's Disc.			3. At edge of Sun's Disc.		
No.		N. Pole.	Equator.	S. Pole.	N. Pole.	Equator.	S. Pole.
No. 1.	100·0	38·8	48·4	58·1	18·7	30·2	28·2
No. 2.	100·0	52·8	.....	56·6	30·5	.....	41·0

Hence it is likewise seen that on May 9th the chemical brightness of the south polar regions was considerably greater than that of the north polar regions, whilst about the equator the brightness was between that of the poles.

In order to show that the sensitive paper, when exposed to ordi-

\* Abstract, *Phil. Mag. S. 4. vol. xxvi. p. 151*; *Memoir, Phil. Trans. 1863.*

† *Astron. Nachr. Nos. 806, 833.*

‡ Kindly placed at my disposal by S. W. Williamson, Esq., of Manchester.

nary sunlight, becomes homogeneously tinted, the author appends the readings, taken in the way described, from various portions of a piece of the standard paper used for the sun-pictures exposed for some seconds to direct sunlight.

		Reading.		Deviation from mean.
Portion No. 1	..	101·4	....	+0·93
„ 2	..	100·7	....	+0·23
„ 3	..	98·5	....	—1·97
„ 4	..	101·6	....	+1·13
„ 5	..	99·9	.. .	—0·57
„ 6	..	100·7	....	+0·23
Mean ..		<u>100·47</u>		

The sun-pictures obtained on the sensitive paper must possess only a slight tint, otherwise the differences in shade cannot be accurately observed; they then exhibit a peculiar coarse mottled appearance, which is not due to imperfections in the paper or the lenses, nor to the action of the earth's atmosphere.

Perhaps these irregular dark and light patches are owing to clouds in the solar atmosphere, and they may have an intimate connexion with the well-known phenomenon of the red prominences.

Mr. Baxendell and the author propose to carry out, according to this method, a regular series of observations of the variation of the relative amounts of brightness on the sun's disc, and they hope before long to be able to present the Society with some further details.

March 17, 1864.—Major-General Sabine, President, in the Chair.

The following communication was read:—

“Description of an Improved Mercurial Barometer.” By James Hicks, Esq.

Having shown this instrument to Mr. Gassiot, he wished me to write a short description of it, which he thought would be of interest to the Royal Society.

Some time since I constructed an open-scale barometer, with a column of mercury placed in a glass tube hermetically sealed at the top, and perfectly open at the bottom. The lower half of the tube is of larger bore than that of the upper.

If a column of mercury, of exactly the length which the atmosphere is able at the time to support, were placed in a tube of glass hermetically sealed at the top, of equal bore from end to end, the mercury would be held in suspension; but immediately the pressure of the atmosphere increased, the mercury would rise towards the top of the tube, and remain there till, on the pressure decreasing, it would fall towards the bottom, and that portion which the atmosphere was unable to support would drop out. But if the lower half of the tube be made a little larger in the bore than the upper, when the column falls, the upper portion passes out of the smaller part of the tube into the larger, and owing to the greater capacity of the latter, the lower end of the column of mercury does not sink to the



same extent as the upper end, and the column thus becomes shorter. The fall will continue until the column is reduced to that length which the atmosphere is capable of supporting, and the scale attached thus registers what is ordinarily termed the height of the barometer.

From the above description it will be evident that, by merely varying the proportion in the size of the two parts of the tube, a scale of any length can be obtained. For example, if the tubes are very nearly the same size in bore, the column has to pass through a great distance before the necessary compensation takes place, and we obtain a very long scale, say 10 inches, for every 1-inch rise and fall in the ordinary barometer. But if the lower tube is made much larger than the upper, the mercury passing into it quickly compensates, and we obtain a small scale, say from 2 to 3 inches, for every inch. To ascertain how many inches this would rise and fall for an ordinary inch of the barometer, I attach it, in connexion with a standard barometer, to an air-pump receiver, and by reducing the pressure in the air-pump I cause the standard barometer to fall, say 1 inch, when the other will fall, say 5 inches; and so I ascertain the scale for every inch, from 31 to 27 inches.

It was on this principle that I constructed the open-scale barometer, which has since been extensively used. But having been asked to apply a vernier to one of these barometers graduated in this way, I found this impracticable, as each varied in length in proportion as the bore of the tube varied, so that every inch was of a different length.

I have now remedied this defect, and made what I believe is an absolute standard barometer, by graduating the scale from the centre, and reading it off with two verniers to the  $\frac{1}{1000}$ th of an inch. The scale is divided from the centre, up and down, into inches, and subdivided into 20ths.

To ascertain the height of the barometer graduated in this way, take a reading of the upper surface of the column of mercury with the vernier, then of the lower surface in the same way, and the two readings added together will give the exact length of the column of mercury supported in the air, which is the height of the barometer at the time.

There is another advantage in this manner of graduating over the former, that if a little of the mercury drops out it will give no error, as the column will immediately rise out of the larger tube into the smaller, and become the same length as before; but by the former scale the barometer would stand too high, until readjusted, which could only be effected by putting the same quantity of mercury in again.

I have introduced Gay-Lussac's pipette into the centre of the tube, to prevent the possibility of any air passing up into the top.



## ROYAL INSTITUTION OF GREAT BRITAIN.

[Continued from p. 306.]

March 4, 1864.—“On the Discrimination of Organic Bodies by their Optical Properties.” By Professor G. G. Stokes, M.A., D.C.L., Sec. R.S.

The chemist who deals with the chemistry of inorganic substances has ordinarily under his hands bodies endowed with very definite reactions, and possessing great stability, so as to permit of the employment of energetic reagents. Accordingly he may afford to dispense with the aids supplied by the optical properties of bodies, though even to him they might be of material assistance. The properties alluded to are such as can be applied to the scrutiny of organic substances; and therefore the examination of the bright lines in flames and incandescent vapours is not considered. This application of optical observation, though not new in principle (for it was clearly enunciated by Mr. Fox Talbot more than thirty years ago), was hardly followed out in relation to chemistry, and remained almost unknown to chemists until the publication of the researches of Professors Bunsen and Kirchhoff, in consequence of which it has now become universal.

But while the chemist who attends to inorganic compounds may confine himself without much loss to the generally-recognized modes of research, it is to his cost that the organic chemist, especially one who occupies himself with proximate analysis, neglects the immense assistance which in many cases would be afforded him by optical examination of the substances under his hands. It is true that the method is of limited application, for a great number of substances possess no marked optical characters; but when such substances do present themselves, their optical characters afford facilities for their chemical study of which chemists generally have at present little conception.

Two distinct objects may be had in view in seeking for such information as optics can supply relative to the characters of a chemical substance. Among the vast number of substances which chemists have now succeeded in isolating or preparing, and which in many cases have been but little studied, it often becomes a question whether two substances, obtained in different ways, are or are not identical. In such cases an optical comparison of the bodies will either add to the evidence of their identity, the force of the additional evidence being greater or less according as their optical characters are more or less marked, or will establish a difference between substances which might otherwise erroneously have been supposed to be identical.

The second object is that of enabling us to follow a particular substance through mixtures containing it, and thereby to determine its principal reactions before it has been isolated, or even when there is small hope of being able to isolate it; and to demonstrate the existence of a common proximate element in mixtures obtained from two different sources. Under this head should be classed the detec-

tion of mixtures in what were supposed to be solutions of single substances\*.

Setting aside the labour of quantitative determinations carried out by well-recognized methods, the second object is that the attainment of which is by far the more difficult. It involves the methods of examination required for the first object, and more besides; and it is that which is chiefly kept in view in the present discourse.

The optical properties of bodies, properly speaking, include every relation of the bodies to light; but it is by no means every such relation that is available for the object in view. Refractive power, for instance, though constituting, like specific gravity, &c., one of the characters of any particular pure substance, is useless for the purpose of following a substance in a mixture containing it. The same may be said of dispersive power. The properties which are of most use for our object are, first absorption, and secondly fluorescence.

Colour has long been employed as a distinctive character of bodies; as, for example, we say that the salts of oxide of copper are mostly blue. The colour, however, of a body gives but very imperfect information respecting that property on which the colour depends; for the same tint may be made up in an infinite number of ways from the constituents of white light. In order to observe what it is that the body does to each constituent, we must examine it in a pure spectrum. [The formation of a pure spectrum was then explained, and such a spectrum was formed on a screen by aid of the electric light. On holding a cell containing a salt of copper in front of the screen, and moving it from the red to the violet, it was shown to cast a shadow in the red as if the fluid had been ink, while in the blue rays it might have been supposed to have been water. Chromate of potash similarly treated gave the reverse effect, being transparent in the red, and opaque in the blue. Of course the transition from transparency to opacity was not abrupt; and for intermediate colours the fluids caused a partial darkening. Indeed, to speak with mathematical rigour, the darkening is not absolute even when it appears the greatest; but the light let through is so feeble that it eludes our senses. In this way the behaviour of the substance may be examined with reference to the various kinds of light one after another; but in order to see at one glance its behaviour with respect to all kinds, it is merely requisite to hold the body so as to intercept the whole beam which forms the spectrum—to place it, for instance, immediately in front of the slit.]

To judge from the two examples just given, it might be supposed that the observation of the colour would give almost as much information as analysis by the prism. [To show how far this is from being the case, two fluids very similar in colour, port-wine and a solution of blood, were next examined. The former merely caused a general absorption of the more refrangible rays; the latter exhibited two

\* The detection of mixtures by the microscopic examination of intermingled crystals properly belongs to the first head, the question which the observer proposes to himself being, in fact, whether the pure substances forming the individual crystals are or are not identical.



well-marked dark bands in the yellow and green.] These bands, first noticed by Hoppe, are eminently characteristic of blood, and afford a good example of the facilities which optical examination affords for following a substance which possesses distinctive characters of this nature. On adding to a solution of blood a particular salt of copper (any ordinary copper salt, with the addition of a tartrate to prevent precipitation, and then carbonate of soda), a fluid is obtained utterly unlike blood in colour, but showing the characteristic bands of blood, while at the same time a good deal of the red is absorbed, as it would be by the copper salt alone. On adding, on the other hand, acetic acid to a solution of blood, the colour is merely changed to a browner red, without any precipitate being produced. Nevertheless, in the spectrum of this fluid the bands of blood have wholly vanished, while another set of bands less intense, but still very characteristic, make their appearance. This alone, however, does not decide whether the colouring matter is decomposed or not by the acid; for as blood is an alkaline fluid, the change might be supposed to be merely analogous to the reddening of litmus. To decide the question, we must examine the spectrum when the fluid is again rendered alkaline, suppose by ammonia, which does not affect the absorption bands of blood. The direct addition of ammonia to the acid mixture causes a dense precipitate, which contains the colouring matter, which may, however, be separated by the use merely of acetic acid and ether, of which the former has been already used, and the latter does not affect the colouring matter of blood. This solution gives the same characteristic spectrum as blood to which acetic acid has been added; but now there is no difficulty in obtaining the colouring matter in an ammoniacal solution. In the spectrum of this solution, the sharp absorption bands of blood do not appear, but instead thereof there is a single band a little nearer to the red, and comparatively vague [this was shown on a screen]. This difference of spectra decides the question, and proves that hæmatine (the colouring matter prepared by acid, &c.) is, as Hoppe stated, a product of decomposition.

The spectrum of blood may be turned to account still further in relation to the chemical nature of that substance. The colouring matter contains, as is well known, a large quantity of iron; and it might be supposed that the colour was due to some salt of iron, more especially as some salts of peroxide of iron, sulphocyanide for instance, have a blood-red colour. But there is found a strong general resemblance between salts of the same metallic oxide as regards the character of their absorption. Thus the salts of sesquioxide of uranium show a remarkable system of bands of absorption in the more refrangible part of the spectrum. The number and position of the bands differ a little from one salt to another; but there is the strongest family likeness between the different salts. Salts of sesquioxide of iron in a similar manner have a family likeness in the vagueness of the absorption, which creeps on from one part of the spectrum to another without presenting any rapid transitions from comparative transparency to opacity and the converse.



[The spectrum of sulphocyanide of peroxide of iron was shown, for the sake of contrasting with blood.] Hence the appearance of such a peculiar system of bands of absorption in blood would negative the supposition that its colour is due to a salt of iron as such, even had we no other means of deciding. The assemblage of the facts with which we are acquainted seems to show that the colouring matter is some complex compound of the five elements, oxygen, hydrogen, carbon, nitrogen, and iron, which, under the action of acids and otherwise, splits into hæmatine and an albuminous substance.

This example was dwelt on, not for its own sake, but because general methods are most readily apprehended in their application to particular examples. To show one example of the discrimination which may be effected by the prism, the spectra were exhibited of the two kinds of red glass which (not to mention certain inferior kinds) are in common use, and which are coloured, one by gold, and the other by suboxide of copper. Both kinds exhibit a single band of absorption near the yellow or green; but the band of the gold glass is situated very sensibly nearer to the blue end of the spectrum than that of the copper glass.

In the experiments actually shown, a battery of fifty cells and complex apparatus were employed, involving much trouble and expense. But this was only required for projecting the spectra on a screen, so as to be visible to a whole audience. To see them, nothing more is required than to place the fluid to be examined (contained, suppose, in a test-tube) behind a slit, and to view it through a small prism applied to the naked eye, different strengths of solution being tried in succession. In this way the bands may be seen by anyone in far greater perfection than when, for the purpose of a lecture, they are thrown on a screen.

In order to be able to examine the peculiarities which a substance may possess in the mode in which it absorbs light, it is not essential that the substance should be in solution, and viewed by transmission. Thus, for example, when a pure spectrum is thrown on a sheet of paper painted with blood, the same bands are seen in the yellow and green region as when the light is transmitted through a solution of blood, and the spectrum thrown on a white screen. This indicates that the colour of such a paper is in fact due to absorption, although the paper is viewed by reflected light. Indeed, by far the greater number of coloured objects which are presented to us, such as green leaves, flowers, dyed cloths, though ordinarily seen by reflexion, owe their colour to absorption. The light by which they are seen is, it is true, reflected, but it is not *in reflexion* that the preferential selection of certain kinds of rays is made which causes the objects to appear coloured. Take, for example, red cloth. A small portion of the incident light is reflected at the outer surfaces of the fibres, and this portion, if it could be observed alone, would be found to be colourless. The greater part of the light penetrates into the fibres, when it immediately begins to suffer absorption on the part of the colouring matter. On arriving at the second surface of the fibre, a portion is reflected and a portion passes on, to be afterwards reflected

from, or absorbed by, fibres lying more deeply. At each reflexion the various kinds of light are reflected in as nearly as possible the same proportion; but in passing across the fibres, in going and returning, they suffer very unequal absorption on the part of the colouring matter, so that in the aggregate of the light perceived the different components of white light are present in proportions widely different from those they bear to each other in white light itself, and the result is a vivid colouring.

There are, however, cases in which the different components of white light are reflected with different degrees of intensity, and the light becomes coloured by regular reflexion. Gold and copper may be referred to as examples. In ordinary language we speak of a soldier's coat as red, and gold as yellow. But these colours belong to the substances in two totally different senses. In the former case the colouring is due to absorption, in the latter case to reflexion. In the same sense, physically speaking, in which a soldier's coat is red, gold is not yellow but blue or green. Such is, in fact, the colour of gold by transmission, and therefore as the result of absorption, as is seen in the case of gold-leaf, which transmits a bluish green light, or of a weak solution of chloride of gold after the addition of protosulphate of iron, when the precipitated metallic gold remains in suspension in a finely-divided state, and causes the mixture to have a blue appearance when seen by transmitted light. In this case we see that while the substance copiously reflects and intensely absorbs rays of all kinds, it more copiously reflects the less refrangible rays, with respect to which it is more intensely opaque.

All metals are, however, highly opaque with regard to rays of all colours. But certain non-metallic substances present themselves which are at the same time intensely opaque with regard to one part of the spectrum, and only moderately opaque or even pretty transparent with regard to another part. Carthamine, murexide, and platinocyanide of magnesium may be mentioned as examples. Such substances reflect copiously, like a metal, those rays with respect to which they are intensely opaque, but more feebly, like a vitreous substance, those rays for which they are tolerably transparent. Hence, when white light is incident upon them the regularly-reflected light is coloured, often vividly, those colours preponderating which the substance is capable of absorbing with intense avidity. But perhaps the most remarkable example known of the connexion between intense absorption and copious reflexion occurs in the case of crystals of permanganate of potash. These crystals have a metallic appearance, and reflect a greenish light. They are too dark to allow the transmitted light to be examined; and even when they are pulverized, the fine purple powder they yield is too dark for convenient analysis of the transmitted light. But the splendid purple solution which they yield may be diluted at pleasure, and the analysis of the light transmitted by it presents no difficulty. The solution absorbs principally the green part of the spectrum; and when it is not too strong, or used in too great thickness, five bands of absorption, indicating minima of transparency, make their appear-

ance [these were shown on a screen]. Now, when the green light reflected from the crystals is analyzed by a prism, there are observed *bright* bands, indicating maxima of reflecting power, corresponding in position to the *dark* bands in the light transmitted by the solution. The fifth bright band, indeed, can hardly, if at all, be made out; but the corresponding dark band is both less strong than the others and occurs in a fainter part of the spectrum. When the light is reflected at a suitable angle, and is analyzed both by a Nicol's prism, placed with its principal section in the plane of incidence, and by an ordinary prism, the whole spectrum is reduced to the bands just mentioned. The Nicol's prism would under these circumstances extinguish the light reflected from a vitreous substance, and transmit a large part of the light reflected from a metal. Hence we see that as the refrangibility of the light gradually increases, the substance changes repeatedly, as regards the character of its reflecting power, from vitreous to metallic and back again, as the solution (and therefore it may be presumed the substance itself) changes from moderately to intensely opaque, and conversely.

These considerations leave little doubt as to the chemical state of the copper present in a certain glass which was exhibited. This glass was coloured only in a very thin stratum on one face. By transmission it cut off a great deal of light, and was bluish. By reflexion, especially when the colourless face was next the eye, it showed a reddish light visible in all directions, and having the appearance of coming from a fine precipitate, though it was not resolved by the microscope, at least with the power tried. It evidently came from a failure in an attempt to make one of the ordinary red glasses coloured by suboxide of copper, and the only question was as to the state in which the copper was present. It could not be oxide, for the quantity was too small to account for the blueness, and in fact the glass became sensibly colourless in the outer flame of a blowpipe. Analysis of the transmitted light by the prism showed a small band of absorption in the place of the band seen in those copper-red glasses which are not too deep; and therefore a small portion of copper was present in the state of suboxide, *i. e.* a silicate of that base. The rest was doubtless present as metallic copper, arising from over-reduction in the manufacture; and accordingly the blue colour, which would have been purer if the suboxide had been away, indicates the true colour of copper by transmitted light, quite in conformity with what we have seen in the case of gold. Hence, in both metals alike, the absorbing and the reflecting powers are, on the whole, greater for the less than for the more refrangible colours, the law of variation with refrangibility being of course somewhat different in the two cases.

Time would not permit of more than a very brief reference to the second property to which the speaker had referred as useful in tracing substances in impure solutions—that of fluorescence. The phenomenon of fluorescence consists in this, that certain substances, when placed in rays of one refrangibility, emit during the time of exposure compound light of lower refrangibility. When a pure



fluorescent substance (as distinguished from a mixture) is examined in a pure spectrum, it is found that on passing from the extreme red to the violet and beyond, the fluorescence commences at a certain point of the spectrum, varying from one substance to another, and continues thence onwards, more or less strongly in one part or another according to the particular substance. The colour of the fluorescent light is found to be nearly constant throughout the spectrum. Hence, when in a solution presented to us, and examined in a pure spectrum, we notice the fluorescence taking, as it were, a fresh start, *with a different colour*, we may be pretty sure that we have to deal with a mixture of two fluorescent substances.

It might be inferred *à priori*, that fluorescence at any particular part of the spectrum would necessarily be accompanied by absorption, since otherwise there would be a creation of *vis viva*; and experience shows that rapid absorption (such as corresponds to a well-marked minimum of transparency indicated by a determinate band of absorption in the transmitted light) is accompanied by copious fluorescence. But experience has hitherto also shown, what could not have been predicted, and may not be universally true\*, that conversely, absorption is accompanied, in the case of a fluorescent substance, by fluorescence.

From what precedes it follows that the colour of the fluorescent light of a solution, even when the incident light is white, or merely sifted by absorption, may be a useful character. To illustrate this, the electric light, after transmission through a deep-blue glass, was thrown on solutions in weak ammonia of two crystallized substances, *æsculine* and *fraxine*, obtained from the bark of the horse-chestnut; the latter of the two occurs also in the bark of the ash, in which, indeed, it was first discovered. Both solutions exhibited a lively fluorescence; but the colour was different, being blue in the case of *æsculine*, and bluish green in the case of *fraxine*. A purified solution obtained from the bark exhibits a fluorescence of an intermediate colour, which would suffice to show that *æsculine* would not alone account for the fluorescence of the solution of the bark.

When a substance possesses well-marked optical properties, it is in general nearly as easy to follow it in a mixture as in a pure solution. But if the problem which the observer proposes to himself be, Given a solution of unknown substances which presents well-

\* Fluorescent substances, like others, doubtless absorb the invisible heat-rays lying beyond the extreme red, in a manner varying from one substance to another. Hence, if we include such rays in the incident spectrum, we have an example of absorption not accompanied by fluorescence. But the invisible heat-rays differ from those of the visible spectrum (as there is every reason to believe) only in the way that the visible rays of one part of the spectrum differ from those of another, that is, by wave-length, and consequently by refrangibility, which depends on wave-length. Hence it is not improbable that substances may be discovered which absorb the visible rays in some parts of the spectrum less refrangible than that at which the fluorescence commences; and *mixtures* possessing this property may be made at pleasure. Nevertheless the speaker has not yet met with a pure fluorescent substance which exhibits this phenomenon.



marked characters with reference to different parts of the spectrum, to determine what portion of these characters belongs to one substance, and what portion to another, it presents much greater difficulties. It was with reference to this subject that the second of the objects mentioned at the beginning of the discourse had been spoken of as that the attainment of which was by far the more difficult. The problem can, in general, be solved only by combining processes of chemical separation, especially fractional separation, with optical observation. When a solution has thus been sufficiently tested, those characters which are found always to accompany one another, in, as nearly as can be judged, a constant proportion, may, with the highest probability, be regarded as belonging to one and the same substance. But while a combination of chemistry and optics is in general required, important information may sometimes be obtained from optics alone. This is especially the case when one at least of the substances present is at the same time fluorescent and peculiar in its mode of absorption.

To illustrate this the case of chlorophyll was referred to. An eminent French chemist, M. Frémy, proposed to himself to examine whether the green colour were due to a single substance, or to a mixture of a yellow and a blue substance. By the use of merely neutral bodies, he succeeded in separating chlorophyll into a yellow substance, and another which was green, but inclining a little to blue; but he could not in this way get further in the direction of blue. He conceived, however, that he had attained his object by dissolving chlorophyll in a mechanical mixture of ether and hydrochloric acid, the acid on separation showing a fine blue colour, while the ether was yellow. Now solutions of chlorophyll in neutral solvents, such as alcohol, ether, &c., show a lively fluorescence of a blood-red colour; and when the solution is examined in a pure spectrum, the red fluorescence, very copious in parts of the red, comparatively feeble in most of the green, is found to be very lively again in the blue and violet. Now a substance of a pure yellow colour, and exercising its absorption therefore, as such substances do, on the more refrangible rays, would not show a pure red fluorescence. Either it would be non-fluorescent, or the fluorescence of its solution would contain (as experience shows) rays of refrangibilities reaching, or nearly so, to the part of the spectrum at which the fluorescence, and therefore the absorption, commences; and therefore the fluorescent light could not be pure red, as that of chlorophyll is found to be even in the blue and violet. The yellow substance separated by M. Frémy, by the aid of neutral reagents, is, in fact, non-fluorescent. Hence the powerful red fluorescence in the blue and violet can only be attributed to the substance exercising the well-known powerful absorption in the red, which substance must therefore powerfully absorb the blue and violet. We can affirm, therefore, *à priori*, that if this substance were isolated it would *not* be blue, but only a somewhat bluer green. The blue solution obtained by M. Frémy owes in fact its colour to a product of decomposition, which when dissolved in neutral solvents is not blue at all, but of a nearly neutral tint, showing, however, in its spectrum extremely sharp bands of absorption.

### LXIII. *Intelligence and Miscellaneous Articles.*

ON THE PASSAGE OF RADIANT HEAT THROUGH ROCK-SALT, EITHER POLISHED, ROUGH, OR COVERED WITH LAMPBLACK, AND ON THE DIFFUSION OF CALORIFIC RAYS. BY H. KNOBLAUCH.

THE following are the results at which Prof. Knoblauch has arrived after a complete investigation of the subject.

#### I.

1. Limpid and chemically pure rock-salt allows all kinds of calorific rays to traverse it in equal proportions, whether the difference in the rays arises from—

- (a) Having been diffusively reflected from different bodies;
- (b) Having traversed different diathermanous bodies;
- (c) Having emanated from different sources of heat.

2. It has been confirmed that after this passage, which exerts the same influence on all elementary rays, the maximum heat in the solar spectrum of a rock-salt prism is in the obscure space beyond the red; within the visible part of the spectrum the calorific conditions are the same for a prism of rock-salt and of flint glass.

#### II.

1. If rock-salt be used, whether unpolished or turbid, the calorific rays from the sun pass in less quantity than those from an Argand lamp, and the latter, in general, in less quantity than those from a calorific source at 100° C.

Increase of roughness enfeebles the passage of all kinds of rays; but it acts most strongly upon solar heat, less upon that of the lamp, and still less upon that from a source of obscure heat.

2. Eliminating the fact of elective absorption (thermocroce) of the substance, the rough surface of unpolished glasses, and the internal cloudiness of milky glasses exert corresponding effects.

3. These phenomena are not to be attributed (as by Mr. Forbes) either to an absorption unequally exerted on calorific rays of different quality, or (as M. Melloni has done) to a dispersion by the rough or turbid media, unequal in extent and dependent on the calorific colour, the effect of which dispersion would be to deflect more or less the rays of the thermoscope. Nor is the roughness of the surface itself, or the direction of the rays emanating from a single point, the determining condition.

4. Heat diffused either by passing through rough or turbid screens, or by reflexion from an unpolished surface, traverses diffusing screens in proportions greater (a) as the rays are more diffuse, (b) as the screens are more diffusive for parallel rays.

5. In fact the really determining condition of the passage through these screens is whether the incident rays are parallel, or more or less variously radiated from a greater or less number of points.

Hence, for one and the same source of heat, the proportion of rays which pass (eliminating a constant quantity of heat which falls directly on the plate) diminishes as the distance from the source increases,—and the more rapidly the greater the diffusive power of the screen.

7. By conveniently arranging the experiment, it is possible to cause this transmission in greater abundance of the rays from a source at  $100^{\circ}$ , compared with those of a lamp (mentioned in No. 1), to disappear, and even, conversely, to bring about a greater transmission of the heat of the lamp.

### III.

1. In the passage of calorific rays through rock-salt covered by lampblack, an elective absorption (suspected by Melloni) is produced. A diffusive effect (supposed by Forbes) is never produced by the rough surface of the layer of lampblack, but sometimes in consequence of a tarnishing of the plate of salt during its being blackened.

2. In the transmission of rays through thin metallic layers deposited upon glass, an elective absorption is produced without diffusion.

3. The most certain manner of recognizing the existence of an elective absorption during the transmission, is to see if the heat before and after its passage through the substance in question retains unaltered its faculty of traversing other diathermanous substances (limpid and polished), or if this faculty is modified.

4. The best means of recognizing an action of diffusion are the following:—

(a) If solar heat is made to pass through the screen examined, and the rays transmitted are compared with direct rays, either the two pencils pass through colourless rock-salt in the same proportion, or the first pencil preponderates over the second. In the latter case the plate operated upon is diffusive.

(b) If, of two identical thermal pencils, one of which consists of parallel and the other of diffused rays, the latter passes most readily through the substance examined, this substance is diffusive. This method gives also a means of comparing the different degrees of diffusion (within wide limits).

### IV.

1. (a) In general, when the angle formed by the calorific rays with a rough or turbid plate diminishes, the diffusion produced increases. This increase with the change in inclination, in the first place becomes larger with the generally diffusive property of the screen, but then again grows less to such a degree that in very rough and sufficiently cloudy plates, just as in the case of clear ones, no difference can be detected in the behaviour of rays which are transmitted at different angles of inclination.

(b) On the contrary, the diffusion produced by reflexion from an

unpolished surface diminishes as the angle of incidence increases, and passes finally into regular reflexion.

2. Between polished plates, and plates roughened on their two surfaces, there are intermediate states such that a calorific coloration of the transmitted rays is produced, quite independently of the effect produced in the interior of the substance, and solely in consequence of the mechanical properties of the surface.

3. Hence in reference to unpolished or turbid media, it is necessary to discriminate between a diffusion which always takes place, and an elective absorption which sometimes occurs.

4. Fused common salt produced a diffusion, but no calorific coloration.

5. A piece of old rock-salt, found to be chemically and mechanically impure, exerted a diffusive action as well as an elective absorption. Analogous conditions explain the divergent observations which have been made in various experiments with rock-salt.—Poggen-dorff's *Annalen*, vol. cxx. p. 177.

#### ON THE VARIATION OF MAGNETIC FORCE WITH THE TEMPERATURE. BY M. MAURITIUS.

The subject of the author's researches is, in the first place, the variation of permanent magnets by repeated changes of temperature; and secondly, the variation of the temporary magnetism of iron, starting from a very elevated temperature.

*Permanent Magnets.*—The method of experimenting is that employed by M. Wiedemann in researches on the same subject: the magnetized bar is placed perpendicularly to the magnetic meridian in the horizontal plane of a moveable magnet whose angular deviations are observed by reflexion. The deviation of the moveable magnet from the magnetic meridian gives the measure of the force of the magnetized bar.

The magnet, the temperature of which is to be varied, is enclosed in a zinc tube which can be put in communication either with a steam-boiler, or with a vessel of water cooled to  $0^{\circ}$ . After having caused the current of steam or cold water to pass through the tube for five minutes, the angular position of the moveable magnet is observed.

The experiments consisted, in the first place, in producing alternately the temperatures of  $100^{\circ}$  and  $0^{\circ}$  until the magnetism of the bar became sensibly constant on the return of the same temperature; and secondly, in submitting the same bar to this process several times successively, magnetizing it afresh after each series of observations. Five magnets of tempered steel were successively used; they were parallelopipedons in shape, and of different sizes, and were magnetized by friction.

*Results.*—When the magnetism of a magnet becomes constant after having been submitted to a great number of changes of temperature, the diminution of its magnetism from  $0^{\circ}$  to  $100^{\circ}$  is propor-



tional to its magnetism at  $0^{\circ}$ . With a magnet A, the ratio  $\frac{M_0 - M_{100}}{M_0}$  was found equal to  $\frac{1}{10.8}$ ,  $\frac{1}{10.6}$ , and  $\frac{1}{10.5}$  in three series, for values of  $M_0$  varying nearly as 1, 2, and 4. With another magnet D, the same ratio has the values  $\frac{1}{4.7}$ ,  $\frac{1}{4.9}$ , and  $\frac{1}{5.0}$  for successive magnetizations.

The value of the ratio found constant for the same bar, whatever may be the intensity of its magnetism, depends, as is well known, on the form of the bar, on the nature of the air, and on the tempering. The author's researches are not sufficient for the estimation of the influence of these different elements. A single bar B was annealed, when the ratio mentioned above became equal to one-fifteenth to one-tenth of what it was before. This experiment agrees with a slight diminution which the ratio undergoes in successive series, because the tempering also diminishes a little. With one bar A, the ratio increased; and it is to be observed that this bar had been submitted to fourteen alterations of temperature before the first series, from which it may be considered that the tempering had become stationary. According to M. Wiedemann, when a magnet returns to the initial temperature, after having been submitted to a high temperature, the diminution of the magnetic force is proportional to the force itself, and an apparent non-agreement with this rule may result from a change in the tempering. According to M. Mauritius, the experiments made with the bar A contradict this. In fact the author finds for the ratio  $\frac{M_0 - M'_0}{M_0}$ , which should be

constant according to M. Wiedemann, the values  $\frac{1}{39}$ ,  $\frac{1}{45}$ , and  $\frac{1}{51}$  after the first elevation of temperature in each of the three series with A, of which the tempering, as mentioned above, had become constant.

*Temporary Magnetism.*—The method of measuring is the same, and the bar, the temporary magnetism of which is to be studied, is placed in an electro-magnetic coil arranged in the same way as the permanent magnet in the preceding experiments. Each experiment is made in the following manner. The current is passed through the coil and the deviation measured; the red-hot bar of iron is suspended inside the coil; and starting from this moment, the position of the moveable magnet is observed, noting the moment at which each observation is made. By this means the variation, with the time, of the temporary magnetism of the bar of iron is measured. Four different bars were employed—one of wrought iron, two of cast iron, and one of cast steel.

*Results.*—At a bright red heat none of the bars were magnetic. Approximate determinations, by immersion of the bars in water, of the temperature at which magnetism began to be manifested, gave  $1000^{\circ}$ .

With the steel bar the increase of magnetic power takes place at

first very rapidly; then for a certain time it goes on slowly; and then again follows a period of rapid augmentation.

With the cast-iron bars, the second period of rapid increase is also observed, but in a less marked degree, but with the wrought iron it does not exist.

The author believes he may conclude from his experiments, that the magnetic properties of iron are developed suddenly at a determinate temperature. This might be caused, according to M. Mauritius, by a sudden change in its chemical constitution; that is to say, at a certain temperature iron may pass from the biatomic to the monatomic condition, and the biatomic condition is that in which magnetism can alone exist. Further, the second period of rapid increase of magnetism may be due to a similar change of chemical constitution, in which the carbon would take part.—*Bibliothèque Universelle de Genève*, March 1864; Poggendorff's *Annalen*, November 1863.

#### ON THE ARTIFICIAL PRODUCTION OF ASTERISM.

BY C. A. GRÜEL, OF BERLIN.

The simple method by which the above phenomenon may be produced, in a manner as clear and perfect as is met with in some of the naturally occurring minerals, is as follows.

A clear piece of plate glass is cut in the form of an equilateral triangle, with sides measuring  $1\frac{1}{2}$  to 2 inches, by means of a paper pattern of these dimensions placed underneath. The surface of this triangle is then rubbed backwards and forwards a few times on a sheet of fine emery paper; each of the three sides being successively guided against a metal rule, which at the same time, being pressed on the emery paper, keeps it in position. The feeble striping of the glass surface thus obtained produces accurately the condition of a series of lines crossing at an angle of  $60^\circ$ , which is fulfilled by the similarly directed edges of the groups of microscopic crystals observed in some kinds of mica, &c.

As the diffraction experiments are among the most splendid of optical phenomena, it is conceivable that a similar regular striping, but effected with accurate machines, will produce a six-, eight-, or multifold star according to the angle under which the gratings cross; and the glass piece thus prepared must infinitely excel both in beauty of action and in the production of actual coloured spectres; whereas here, from irregular striping, the spectra are superposed, and only represent the well-known star formed of bright radial lines like the star of mica, &c.\*—Poggendorff's *Annalen*, November 1863.

\* I may mention that G. Rose (Poggendorff's *Annalen*, vol. cxvii. p. 635) has also produced asterism by isinglass impressions of mica.—POGGENDORFF.

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LXIV. *On the Measurement of Heights by the Barometer, and on Atmospheric Refraction, having regard to the Constitution of the Atmosphere, resulting from Mr. James Glaisher's Observations.*  
By Count PAUL DE SAINT-ROBERT\*.

IN a former brief paper, published in the *Philosophical Magazine* for February 1864, I pointed out the incompatibility of the barometrical formula, generally used for the computation of heights, with the constitution of the atmosphere as found by Mr. James Glaisher in his balloon-ascents, and showed the modification that it must undergo in order to take into account the conditions of temperature actually observed in ascending into the atmosphere.

Having since calculated the density of the successive strata of the atmosphere arising out of Mr. Glaisher's results, I have noticed that the density of the air decreases nearly uniformly as the height increases. This law of density being admitted, the determination of altitudes by the barometer and of atmospheric refraction is very much simplified. In fact, to obtain the altitude in that case, it suffices to divide the difference of pressure at the lower and upper stations by half the sum of the specific gravities of the air at the same stations; and the trajectory described by a ray of light passing through an atmosphere so constituted is an arc of hyperbola.

This law of density in the atmosphere was made use of very early by mathematicians in the calculation of atmospheric refraction, on account of the facility that it affords for the integration of the differential equation of refraction, and of the conformity

\* Communicated by the Author.

of the results to which it leads with those observed ; but I think it was never confirmed by direct observations of the density of the successive strata of the atmosphere ; nor do I think that it has been used heretofore in the formula for the measurement of heights by the barometer.

### I. Constitution of the Atmosphere.

The condition of any stratum of the atmosphere depends upon five quantities—namely, the temperature  $t$ , the pressure  $p$ , the elasticity  $\varpi$  of the aqueous vapour, the density  $\rho$ , and the distance  $r$  from the earth's centre. These quantities are linked together by two equations ; of which the first is the equation of *equilibrium*,

$$dp = -g\rho dr, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

expressing that the decrement of pressure in ascending from the distance  $r$  to the distance  $r + dr$  is equal to the weight of the column of air included between  $r$  and  $r + dr$ .

The second is the equation of *dilatibility*,

$$\frac{\rho}{\rho_0} = \frac{t_0}{t} \left( \frac{p - m\varpi}{p_0 - m\varpi_0} \right), \quad . \quad . \quad . \quad . \quad (2)$$

$1 - m$  being the ratio of the density of vapour to that of the air at the same temperature and pressure (according to Gay-Lussac  $m = \frac{2}{3}$ ) ; which equation embodies the laws of Mariotte, Gay-Lussac, and Dalton. The temperature is reckoned from the absolute zero (493°·2 F. below the melting-point of ice).

Now if we could have, either by experiment or by theory, two other relations among the five quantities above mentioned, the first four, which are the physical elements of the aërial strata, would be determinable in function of the altitude alone ; thus the physical constitution of the atmosphere would be entirely known. Were we to neglect the vapour mixed with the air, then only one relation, either theoretical or experimental, added to the two first, would be sufficient to determine the condition of the atmosphere.

The knowledge of these required relations can be experimentally obtained by balloon-ascent, when the barometer, the thermometer, and the hygrometer are observed simultaneously in a great number of strata successively traversed, without any need of knowing the elevation. In fact with the values of  $p$ ,  $t$ , and  $\varpi$  thence resulting, we can deduce for every stratum the density  $\rho$  by means of equation (2) ; then by (1), if the observations are numerous enough, we can by the method of quadratures obtain



the value of the distance  $r$  corresponding to every value of  $\rho$ , and of  $p$ ,  $t$ ,  $\varpi$ . We can thus have all the physical elements of the atmosphere, without having recourse to any hypothesis whatever on the decrease of heat or of density.

Before applying this process of calculation to Mr. Glaisher's results, let us put equation (1) under another form. We shall characterize by an index 0 the quantities which belong to the inferior stratum of air; and we shall introduce an auxiliary constant  $h$ , such that

$$p_0 = g_0 \rho_0 h,$$

whereby  $h$  will be the height of a homogeneous atmosphere of the same state as the air at the lower station.

Taking into account the variation of gravity according to the vertical height, we have

$$g = g_0 \left( \frac{r_0}{r} \right)^2.$$

Wherefore equation (1) becomes

$$d \left( \frac{p}{p_0} \right) = \frac{r_0}{h} \frac{\rho}{\rho_0} d \left( \frac{r_0}{r} \right).$$

Integrating from  $p_0$  to  $p$ , and from  $r_0$  to  $r$ , we get

$$\frac{r_0}{r} (r - r_0) = h \int_p^{p_0} \frac{\rho}{\rho_0} d \left( \frac{p}{p_0} \right);$$

and putting

$$\frac{r_0}{r} (r - r_0) = x,$$

we obtain the following equation instead of (1),

$$x = h \int_p^{p_0} \frac{\rho}{\rho_0} d \left( \frac{p}{p_0} \right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

We must observe that, since in the extent of the atmosphere the value of  $\frac{r_0}{r}$  is always very near unity, it follows that  $x$  represents very nearly the difference of level between the two stations.

The following Table exhibits the mean results obtained by Mr. Glaisher in his eight balloon-ascent, made in 1862\*.

\* An Account of Meteorological and Physical Observations in eight Balloon-Ascents, made at the request of the Committee. By James Glaisher, F.R.S. London, 1863.

TABLE I.—Results of Mr. Glaisher's Observations upon the Temperature and Hygrometric condition of the Air at every 1000 feet of height.

Height calculated by Laplace's formula.	Partially clear sky.		Overcast sky.	
	Decrease of temperature.	Relative humi- dity of the air.	Decrease of temperature.	Relative humi- dity of the air.
feet.	° F.		° F.	
0	0	63	0	77
1,000	7.2	68	4.5	77
2,000	12.5	77	8.7	77
3,000	17.1	76	12.8	80
4,000	20.5	76	16.5	80
5,000	23.2	69	19.6	83
6,000	26.0	68		
7,000	28.8	64		
8,000	31.5	58		
9,000	34.1	52		
10,000	36.7	52		
11,000	39.3	48		
12,000	41.9	48		
13,000	44.4	43		
14,000	46.6	58		
15,000	48.7	53		
16,000	50.8	45		
17,000	52.7	33		
18,000	54.5	21		
19,000	56.3	36		
20,000	57.8	33		
21,000	59.1	32		
22,000	61.4	21		
23,000	62.4	16		
24,000	63.7			
25,000	64.8			
26,000	65.8			
27,000	66.8			
28,000	67.7			
29,000	68.5			
30,000	70.0			

In order to avoid a vicious circle, we must put aside the heights calculated by Laplace's formula grounded on a law of decrement of heat inconsistent with that found by Mr. Glaisher, and we must have the pressure corresponding to each temperature and state of humidity. This is easily done by an inverse operation from that employed by Mr. Glaisher to calculate the heights. The altitude  $z$  being given, the pressure due thereto is found by the formula

$$\log \frac{p}{p_0} = - \frac{z}{60159 \left( 1 + \frac{\theta + \theta' - 64}{900} \right)},$$

$\theta$ ,  $\theta'$  denoting the temperatures of Fahrenheit's thermometer.

In this manner the following Table has been calculated, the corresponding absolute temperature, pressure, and density at the level of the sea being taken each as unity:—

TABLE II.—Showing the Decrease of Temperature, Pressure, and Density in the Atmosphere at different altitudes.

Partially clear sky.						Overcast sky.					
Temperature.	Pressure of moist air.	Pressure of vapour.	Density of moist air.	Height deduced by the method of quadratures.		Temperature.	Pressure of moist air.	Pressure of vapour.	Density of moist air.	Height deduced by the method of quadratures.	
				Without considering the variation of gravity.	Taking into consideration the variation of gravity.					Without considering the variation of gravity.	Taking into consideration the variation of gravity.
				ft.	ft.					ft.	ft.
1.0000	1.0000	0.39	1.0000	0	0	1.0000	1.0000	0.48	1.0000	0	0
0.9863	0.9648	0.33	0.9788	1,002	1,002	0.9914	0.9649	0.41	0.9739	1002	1002
0.9763	0.9305	0.30	0.9540	2,001	2,001	0.9835	0.9308	0.35	0.9476	2002	2002
0.9675	0.8971	0.25	0.9286	3,000	3,000	0.9757	0.8976	0.31	0.9215	3003	3003
0.9610	0.8648	0.22	0.9013	3,994	3,995	0.9686	0.8653	0.27	0.8952	4005	4006
0.9559	0.8335	0.18	0.8738	4,987	4,988	0.9628	0.8341	0.25	0.8683	5002	5003
0.9506	0.8032	0.16	0.8469	5,978	5,980						
0.9453	0.7737	0.14	0.8206	6,974	6,976						
0.9401	0.7453	0.11	0.7951	7,964	7,967						
0.9352	0.7177	0.09	0.7700	8,957	8,961						
0.9303	0.6911	0.08	0.7454	9,945	9,950						
0.9253	0.6652	0.07	0.7215	10,939	10,945						
0.9204	0.6402	0.06	0.6982	11,931	11,938						
0.9156	0.6161	0.05	0.6754	12,919	12,927						
0.9114	0.5928	0.06	0.6527	13,907	13,916						
0.9074	0.5703	0.05	0.6309	14,894	14,905						
0.9035	0.5486	0.04	0.6096	15,879	15,891						
0.8997	0.5277	0.03	0.5890	16,861	16,875						
0.8964	0.5076	0.015	0.5688	17,839	17,854						
0.8930	0.4879	0.02	0.5487	18,832	18,849						
0.8902	0.4694	0.02	0.5296	19,798	19,817						
0.8877	0.4515	0.02	0.5108	20,767	20,788						
0.8853	0.4338	0.01	0.4934	21,760	21,783						
0.8814	0.4172	0.008	0.4756	22,725	22,750						
0.8789	0.4011	...	0.4586	23,696	23,723						
0.8769	0.3857	...	0.4420	24,659	24,688						
0.8750	0.3709	...	0.4260	25,619	25,650						
0.8731	0.3566	...	0.4105	26,582	26,616						
0.8713	0.3429	...	0.3954	27,539	27,575						
0.8698	0.3297	...	0.3809	28,497	28,536						
0.8670	0.3167	...	0.3671	29,476	29,518						

In calculating the first and second columns, the absolute temperature and the pressure at the level of the sea were assumed to

be respectively  $t_0 = 526^\circ.2$ ,  $p_0 = 30$  inches. The third column was obtained by help of Regnault's Table of the elastic force of vapour. The fourth was calculated by equation (2), and the fifth by equation (3). The sixth gives the value of  $r - r_0$ , or of the altitude, taking into consideration the variation of gravity.

The height of a homogeneous atmosphere, at the temperature of melting ice and 30 inches pressure, was taken equal to 26,254 feet; so that the height at the absolute temperature  $t_0$ , at the pressure  $p_0$ , and with the mixture of vapour at the pressure  $\varpi$ , will be

$$h = 26254 \frac{t_0 \rho_0}{493.2 (p_0 - m\varpi_0)}.$$

When the sky was partially clear,

$$t_0 = 526^\circ.2, \quad p_0 = 30 \text{ in.}, \quad \varpi = 0.39 \text{ in.};$$

therefore

$$h = 28,148 \text{ feet.}$$

When the sky was cloudy,

$$t_0 = 526^\circ.2, \quad p_0 = 30 \text{ in.}, \quad \varpi_0 = 0.48 \text{ in.};$$

therefore

$$h = 28,180 \text{ feet.}$$

With these values of  $h$ , the heights  $x$  contained in the fifth column were calculated.

The comparison of Table II. with Table I. shows that the altitudes calculated by Laplace's formula fall short of the real ones, and consequently that it requires a negative correction, which result I had already advanced in my former paper. As far as the height is moderate, the difference is small; but it becomes of consequence for great elevations. The difference at 13,000 feet (which is a little more than the height of Monviso) is already 73 feet; and at 16,000 feet (not much more than the height of Mont Blanc) it becomes 109 feet.

On inspection of Table II., it will be seen that the density of the air decreases at first more rapidly than the increase of height up to about 5000 feet, and after that point it decreases less rapidly than the increase of altitude, but that on the whole the decrease of density is nearly proportional to the increase of elevation.

Many laws of density have been assumed by mathematicians for the purpose of calculating atmospheric refraction. The principal ones are that of Thomas Simpson, assuming the density to decrease uniformly as the height increases; that of Bessel, supposing the density to decrease in a geometrical progression; that of Laplace, partaking both of the arithmetical and geometrical progression of variation of density; that of Ivory, suppo-



sing between the temperature and the density the relation

$$\frac{t}{t_0} = 1 - f \left( 1 - \frac{\rho}{\rho_0} \right),$$

$f$  being a constant depending on the rate at which the temperature decreases as the height increases.

For the purpose of comparison, I subjoin the following Table of the successive densities according to the distances from the earth's surface, as deduced from the various laws just mentioned.

TABLE III.—Showing the Decrease of Density at different Altitudes resulting from various laws.

Height.	Density of the air.								
	Observed.	Uniform decrease.	Difference.	Bessel.	Difference.	Laplace.	Difference.	Ivory.	Difference.
feet.									
0	1·0000	1·0000	0·0000	1·0000	0·0000	1·0000	0·0000	1·0000	0·0000
5,000	0·8734	0·8867	+0·0133	0·8367	-0·0367	0·8649	-0·0085	0·8714	-0·0020
10,000	0·7441	0·7734	+0·0293	0·7000	-0·0441	0·7366	-0·0075	0·7514	+0·0073
15,000	0·6286	0·6601	+0·0315	0·5857	-0·0429	0·6188	-0·0098	0·6434	+0·0148
20,000	0·5257	0·5468	+0·0211	0·4900	-0·0357	0·5151	-0·0126	0·5515	+0·0258
25,000	0·4363	0·4335	-0·0028	0·4100	-0·0263	0·4205	-0·0158	0·4681	+0·0318
30,000	0·3597	0·3202	-0·0395	0·3430	-0·0167	0·3411	-0·0186	0·3939	+0·0342
Sum of the squares of the differences }			0·0040	.....	0·0074	.....	0·0010	.....	0·0031

The column headed "Density of the air observed" is derived from Table II. by means of interpolations in order to have the altitudes in an arithmetical progression. The figures in the third column were calculated by the formula

$$\frac{\rho}{\rho_0} = 1 - ax,$$

in which  $a$ , deduced by the method of least squares, was taken  $a = 0·00002266$ .

The densities resulting from the different hypotheses were calculated respectively by means of the following equations:—

$$\text{Bessel's} \quad \frac{\rho}{\rho_0} = e^{-(0·00003566)x}.$$

$$\text{Laplace's} \quad \begin{cases} \frac{\rho}{\rho_0} = [1 + (661·107)u]e^{-(1348·04)u}, \\ x = (20886860)u + 6138 \left( 1 - \frac{\rho}{\rho_0} \right). \end{cases}$$

$$\text{Ivory's} \quad \begin{cases} \frac{\rho}{\rho_0} = e^{-u}, \\ x = (22384)u + 14922 \left( 1 - \frac{\rho}{\rho_0} \right). \end{cases}$$

A glance at Table III. will enable one to see that the law of uniform decrease is a pretty near approximation to the true law of decrease. We perceive indeed that the diminution of density is slower the higher we ascend, but the law of uniform decrease holds very nearly the whole way from the level of the sea up to 30,000 feet; while Bessel's assumption deviates much more from the results of actual observations. I think that, without looking for a more complicated law, we may well adopt this for calculating by the barometer heights that never reach that limit, and for determining atmospheric refraction, which, as we shall see further on, is for the most part dependent on the first envelope of air not extending very far from the earth's surface.

This identical hypothesis was adopted by Laplace for the determination of terrestrial refraction\*.

I proceed to examine the results of adopting this law in reference to the pressure and temperature of atmospheric strata.

Introducing the law of density,

$$\frac{\rho}{\rho_0} = 1 - ax,$$

in the equation of equilibrium of the atmosphere,

$$d\left(\frac{p}{\rho_0}\right) = -\frac{1}{h} \frac{\rho}{\rho_0} dx,$$

we get, after integration,

$$\frac{p}{\rho_0} = 1 - \frac{1}{2ah} \left[ 1 - \left( \frac{\rho}{\rho_0} \right)^2 \right].$$

At the upper limit of the atmosphere we must have the pressure  $p=0$ ; so that the density of the upper layer of air is given by equation

$$\frac{\rho}{\rho_0} = \sqrt{1 - 2ah}.$$

In order that the density may be a real quantity, as is required by the equilibrium of the upper strata of the atmosphere, it is necessary that  $2ah$  should be inferior to unity, or

$$a < \frac{1}{2h}.$$

Since

$$x = \frac{1}{a} \left( 1 - \frac{\rho}{\rho_0} \right),$$

it follows that the height of the atmosphere would be in that case less than  $2h$ ; that is to say, less than ten or eleven miles,

\* Laplace, *Mécanique Céleste*, vol. iv. p. 277.

which height is no doubt too small, it having been estimated by means of the duration of the twilight to be between forty and fifty miles.

The value we have adopted for  $a$ ,

$$a = 0.00002266,$$

being greater than

$$\frac{1}{2h} = 0.00001776,$$

renders imaginary the value of  $\frac{\rho}{\rho_0}$  correspondent to

$$p = 0.$$

In this case the density becomes nothing at the height

$$x = \frac{1}{a} = 44,131 \text{ feet},$$

which corresponds to a pressure

$$\frac{p}{p_0} = 1 - \frac{1}{2ah} = 0.2161;$$

so that the density is reduced to nothing before the pressure, which is physically impossible. It follows that with this value of  $a$  the equilibrium of the upper strata could not exist. But we must bear in mind that for our purpose of determining altitudes and refractions, it is quite sufficient that the law adopted represents the constitution of the atmosphere in the first envelope of air near the earth's surface, and that it is useless to consider the higher strata, which do not materially affect the inquiries we are about.

Coming now to the temperature, if we neglect the aqueous vapour, or even if we suppose its tension to bear a constant ratio to the tension of dry air, we shall have

$$\frac{p}{p_0} = \frac{\rho t}{\rho_0 t_0};$$

therefore

$$\frac{t}{t_0} = \frac{\rho_0}{\rho} - \frac{1}{2ah} \left( \frac{\rho_0}{\rho} - \frac{\rho}{\rho_0} \right).$$

By substituting the value of  $\rho$  in  $x$ , we shall obtain

$$\frac{t}{t_0} = \frac{2h - 2x + ax^2}{2h(1 - ax)};$$

hence

$$\frac{x}{t_0 - t} = \frac{2h}{t_0} \left( 1 + \frac{2ah - 1}{2 - 2ah - ax} \right).$$

This is the expression of the height necessary to ascend to have a fall of one degree in the temperature. We see that it increases while the elevation increases, as it ought to be in accordance with Mr. Glaisher's results.

We shall now proceed to investigate the formula for the computation of heights by means of the barometer.

## II. *Barometrical Formula.*

The equation of equilibrium of the atmosphere, taking into account the variation of gravity, is

$$dp = -g_0 \left( \frac{r_0}{r} \right)^2 \rho dr,$$

$g_0$  being the gravity at the lower station. Putting, as before,

$$x = \frac{r_0}{r} (r - r_0),$$

we get

$$dp = -g_0 \rho dx.$$

If we assume

$$\rho = \rho_0 (1 - ax),$$

we shall have

$$dp = -g_0 \rho_0 (1 - ax) dx;$$

whence, integrating,

$$p_0 - p = g_0 \rho_0 x \left( 1 - \frac{ax}{2} \right).$$

Eliminating  $a$  between this equation and the last but one, we shall find

$$x = \frac{2}{g_0} \left( \frac{p_0 - p}{\rho_0 + \rho} \right).$$

We could have obtained directly, without help of the infinitesimal calculus, this expression by the simple consideration that the weight of the column of air comprised between the two stations must be equal to the difference of pressure.

As the density of the air is not given directly by the instruments employed in barometrical mensuration, we shall put in place of  $\rho$  its value

$$\rho = \frac{p - m\varpi}{ct}.$$

Consequently we shall have for the barometrical formula,

$$x = \frac{2c}{g_0} \frac{p_0 - p}{\frac{p_0 - m\varpi_0}{t_0} + \frac{p - m\varpi}{t}}.$$



The pressures  $p_0$  and  $p$  are measured by the height of the mercurial column in the barometer at the two stations. If  $H_0$  and  $H$  denote these heights reduced to the freezing-point,  $g_0$  and  $g$  the gravity at the same stations,  $\Delta$  the density of the mercury, we shall have

$$p_0 = g_0 \Delta H_0, \quad p = g \Delta H,$$

and

$$g = g_0 \left( \frac{r_0}{r} \right)^2.$$

The pressures  $\varpi$  and  $\varpi_0$  of the vapour contained in the atmosphere at the upper and lower stations are known from observations of the dew-point, or of the temperature of a thermometer with a wet bulb.

The constant  $c$  is given when we know the density of the air corresponding to a given temperature. It has been found by Regnault that the relative density of mercury and dry air at the freezing-point, under a barometrical pressure of  $0^m.76$ , at the sea-level and at  $45^\circ$  degrees of latitude, is  $10517.3$ . Assuming for the absolute zero  $-274^\circ$  C., we shall have, in metrical measure,

$$c = \frac{p}{\rho t} = \frac{(0.76)\Delta G}{274 \cdot \rho} = \frac{(0.76)(10517.3)G}{274} = (29.172)G;$$

and in English measure,

$$c = (53.173)G,$$

$G = 9^m.80604 = 32.172$  feet (Bessel), being the gravity at the level of the sea in latitude  $45^\circ$ .

If  $\lambda$  be the latitude of the place of observation, we shall have for the gravity there

$$g_0 = G(1 - 0.0026 \cos 2\lambda) \left( \frac{R}{r_0} \right)^2,$$

where  $R = 6,366,786^m = 20,888,780$  feet (Arago, *Astr. Pop.* vol. iii. p. 341) is the earth's radius at the latitude of  $45^\circ$ .

Therefore the complete formula for the measurement of heights is

$$r - r_0 = A(1 + 0.0026 \cos 2\lambda) \left( \frac{r_0}{R} \right)^2 \left( \frac{r}{r_0} \right) \frac{H_0 - H \left( \frac{r_0}{r} \right)^2}{\frac{H_0 - m\eta_0}{t_0} + \frac{H \left( \frac{r_0}{r} \right)^2 - m\eta}{t}},$$

$\eta$  and  $\eta_0$  denoting the elastic force of vapour in metres or inches according to the measure employed. The constant

$$A = 58.344 \text{ in metrical measure,}$$

$$= 106.346 \text{ in English measure.}$$

When the upper station is on the surface of the ground, the factor  $\left(\frac{r_0}{r}\right)^2$  must be augmented in order to take into account the attraction of the portion of the earth which is elevated above the level of the lower station. According to Poisson\*, we must substitute for it the expression

$$\left(\frac{r_0}{r}\right)^2 + \frac{3}{4}\left(\frac{r-r_0}{r_0}\right),$$

or approximately

$$1 - \frac{5}{4}\left(\frac{r-r_0}{r_0}\right).$$

The same remark is applicable to  $\left(\frac{R}{r_0}\right)^2$ , when the lower station is on the surface of the ground.

In ordinary cases the variation of gravity may be neglected, but then the coefficient A should be somewhat increased, for instance about  $\frac{1}{300}$ , similarly to what is done in the ordinary barometrical formula.

When the degree of moisture of the air at the two stations has not been ascertained, we may use the very simple formula

$$x = A \frac{H_0 - H}{\frac{H_0}{t_0} + \frac{H}{t}},$$

increasing somewhat the constant A in order to include the correction for the effect of moisture. An increase of  $\frac{1}{200}$  would correspond to a pressure of vapour at the upper and lower station of  $\frac{1}{75}$  of the respective atmospheric pressure.

The value of the coefficient A, including both corrections (variation of gravity and presence of moisture), would thus become

$$A = 58.8 \text{ in metrical measure,}$$

$$A = 107.2 \text{ in English measure.}$$

### III. *Atmospheric Refraction*.

It is known that if the earth's surface were a plane, and the air consequently disposed in plane and parallel strata, the effect of the atmosphere upon a ray of light passing through it would depend only upon the density of the air at the two ends of the

\* *Traité de Mécanique*, vol. i. No. 255; vol. ii. No. 629.

ray, whatever might be the constitution of the interposed strata. By the simple law of the constancy of the ratio of the sines of incidence and refraction, the amount of astronomical refraction would be immediately known when the density of the air at the place of observation was given by the indications of the barometer and thermometer. The determination of terrestrial refraction would require besides the knowledge of the state of the air at the object.

Owing to the greatness of the earth's radius, its surface is sensibly a plane to a certain extent round the observer, and the layers of air are likewise sensibly plane and parallel to a certain extent round a vertical line passing through the observer; this is the reason why the astronomical refraction, down to about  $74^{\circ}$  zenith-distance, is hardly affected in a perceptible degree by the peculiar constitution of the atmosphere.

Near the horizon the same ray of light encounters the several concentric strata of air in parts which are far from being parallel; hence we cannot determine the deflections caused by them without knowing the arrangement of their density. But happily refraction depends almost entirely upon the lower portion of the atmosphere (which is accessible to exploration), and is not sensibly affected by the higher regions.

Ivory, in a memoir on refraction published in the *Philosophical Transactions of the Royal Society of London* for 1823, first pointed out, as a consequence of his calculations, the singular fact that "the refractions undergo hardly any change in all the atmospheres comprehended in the formula" which he assumed for representing the decrease of density; "although their height increases from about twenty-five miles to be infinitely great"\*.

The physical cause of this fact was afterwards shown by Biot, in a paper on astronomical refraction published in the *Connaissance des Temps* for 1839, to be the following.

Let us consider any luminous trajectory whatever, arriving at the observer at a given zenith-distance; and let us retrace it in our mind, ascending with it into the different strata of air. As we recede from the earth's surface, the angle between the curve and the radius-vector, directed to the centre of the earth, will progressively decrease. It is obvious that in continuing to ascend we must come to an altitude where this angle will not be greater than  $74^{\circ}$ , or whatever other zenith-distance beyond which refraction is no longer influenced by the superincumbent atmosphere, and depends only upon the density at such altitude. Whence we see that refraction will depend only upon the mode

\* *Philosophical Transactions of the Royal Society of London* for 1823, part 2, p. 451.

of constitution of the inferior strata, and in no way upon that of the remainder of the atmosphere.

It might be objected to this explanation of Biot, that, in order to reach the angle of  $74^\circ$ , it is necessary to ascend to a very great elevation. For instance, the luminous trajectory which arrives horizontally at the earth's surface must be retraced to the height of  $\frac{1}{25}$ th of the earth's radius in order to arrive at an angle of incidence of  $74^\circ$ . Very likely the atmosphere does not extend so far. But we must remark that, once arrived at a certain elevation, the extreme tenuity of air renders the amount of refraction so small that it may be well supposed to depend only on the density of the air at that place, though the angle of incidence should not yet be reduced to the limit of  $74^\circ$ .

To exemplify this, let us consider the luminous trajectory arriving horizontally at the observer. By an easy calculation, which I will explain hereafter, we find that at 30,000 feet, the height actually reached by Mr. Glaisher, the angle of incidence becomes reduced to  $87^\circ 8' 25''$ , and that the angle at the earth's centre comprised between the two radii vectores is  $3^\circ 18' 32''$ . It follows that the angle made by the tangent drawn to the trajectory at 30,000 feet with the first tangent, that is to say, the amount of refraction produced by the shell of air of the depth of 30,000 feet, is  $26' 57''$ .

Now, if we were to suppose the strata beyond 30,000 feet to be plane and parallel, by the simple law of refraction, viz. of the invariability of the ratio of the sines, we should find that the light passing directly from vacuum into air of the density of 0.358, which it would be at 30,000 feet, would suffer a deflection of  $7' 25''$ . Adding this to the refraction due to the lower shell of air, we should obtain  $34' 22''$  for the total horizontal refraction.

Had we considered the whole atmosphere, we should have found a refraction of  $33' 37''$ . The difference is only  $45''$ . Going a little further, we should arrive at an altitude where the consideration of the ulterior strata would be unnecessary.

The consequence to be drawn from all this reasoning is, that, for the determination of astronomical refraction, it is sufficient to know the constitution of a shell of air of comparative thinness. Mr. Glaisher's observations furnish us with that knowledge up to 30,000 feet, which is quite sufficient for our purpose. As for terrestrial refraction, we never have to observe terrestrial objects beyond the strata explored by Mr. Glaisher. We are thus in possession of sufficient data for calculating both refractions.

When the law of equable decrease of density upwards from the sea-level is admitted, the path followed by a ray of light is an arc of an hyperbola whose interior focus occupies the centre



of the strata, as I am about to prove. But I will previously make an observation with respect to the aqueous vapour mixed with the air.

Laplace was the first to observe that, if a mixture of vapour with dry air diminishes the refractive power by making the density less, the greater action of the vapour upon light is found almost exactly to compensate the defect. This was confirmed experimentally by Biot and Arago. The consequence of this is, that, as far as the refractive power of the air is concerned, we may substitute dry air for moist at the same temperature and pressure.

According to this principle Table IV. was calculated, in place of Table III., for all that concerns the optical effects of air.

TABLE IV.—Showing the Decrease of Density of the Air, considered as dry, at different Altitudes, and the comparison of various Laws of Decrease.

	Density of the air.								
Height.	Ob- served.	Uniform decrease.	Difference.	Bessel.	Difference.	Laplace.	Difference.	Ivory.	Difference.
feet.									
0	1.0000	1.0000	0.0000	1.0000	0.0000	1.0000	0.0000	1.0000	0.0000
5,000	0.8715	0.8862	+0.0147	0.8367	-0.0348	0.8649	-0.0066	0.8714	-0.0001
10,000	0.7416	0.7724	+0.0308	0.7000	-0.0416	0.7366	-0.0050	0.7514	+0.0098
15,000	0.6262	0.6587	+0.0325	0.5857	-0.0405	0.6188	-0.0074	0.6434	+0.0172
20,000	0.5234	0.5449	+0.0215	0.4900	-0.0334	0.5131	-0.0103	0.5515	+0.0281
25,000	0.4342	0.4311	-0.0031	0.4100	-0.0242	0.4205	-0.0137	0.4681	+0.0339
30,000	0.3580	0.3173	-0.0407	0.3430	-0.0150	0.3411	-0.0169	0.3939	+0.0359
Sum of the squares of the differences }			0.0043	.....	0.0065	.....	0.0007	.....	0.0036

The rate of the decrease deduced from the observed densities by the method of least squares comes out

$$a=0.00002276,$$

which should be used whenever the optical effects are concerned, reserving the value

$$a=0.00002266,$$

previously found, for the conditions of equilibrium depending on weight.

We shall now proceed to the determination of the curve described by light in its passage through the atmosphere.

Let  $r$  denote the radius-vector conducted from the earth's centre to any point of the luminous trajectory;  $v$  the angle which this radius-vector makes with a vertical line passing through the observer;  $\theta$  the angle made by the tangent to the

curve with the same vertical line;  $v'$  the angle made by the tangent with the radius-vector  $r$ ;  $\rho$  the density of the air at the assumed point;  $4k$  the refractive power peculiar to atmospheric air of the density 1; and  $r_0, \theta_0, \rho_0$  the values of  $r, \theta, \rho$  at the point where the trajectory enters the observer's eye.

The determination of the luminous trajectory depends upon the following equation,

$$\frac{r_0 \sin \theta_0}{r \sin v'} = \frac{\sqrt{1 + 4k\rho}}{\sqrt{1 + 4k\rho_0}};$$

where  $r_0 \sin \theta_0, r \sin v'$  are the perpendiculars let fall from the earth's centre upon the tangents, making respectively the angles  $\theta_0, v'$  with the radius-vector.

Introducing in it the assumption

$$\frac{\rho}{\rho_0} = 1 - ax = 1 - ar_0 \left( \frac{r - r_0}{r} \right),$$

and making, for the sake of brevity,

$$\alpha = \frac{2k\rho_0}{1 + 4k\rho_0},$$

we get

$$\frac{1}{(r \sin v')^2} = \frac{2\alpha a}{\sin^2 \theta_0} \cdot \frac{1}{r} + \frac{1 - 2\alpha ar_0}{(r_0 \sin \theta_0)^2}.$$

Now by conic sections we have in the hyperbola

$$\frac{1}{N^2} = \frac{2A}{B^2} \cdot \frac{1}{r} + \frac{1}{B^2},$$

$N$  being the perpendicular from the focus upon the tangent,  $A$  and  $B$  the semiaxes of the hyperbola.

Hence the luminous trajectory is an hyperbola whose focus is in the centre of the earth, and whose semiaxes are

$$A = \frac{\alpha ar_0^2}{1 - 2\alpha ar_0}, \quad B = \frac{r_0 \sin \theta_0}{\sqrt{1 - 2\alpha ar_0}}.$$

The polar equation, about the earth's centre, to the luminous trajectory will then be

$$r = \frac{A(e^2 - 1)}{1 + e \cos \phi},$$

$\phi$  being measured from the vertex, and

$$e^2 = \frac{A^2 + B^2}{A^2} = 1 + \frac{\sin^2 \theta_0}{\alpha^2 a^2 r_0^2} (1 - 2\alpha ar_0).$$

Let  $V$  be the angle made by the major axis of the hyperbola with the observer's vertical line. The angle  $V$ , as well as  $v$ , is measured from the vertical line in the direction that causes the radius-vector to increase. Thus

$$\phi = 180^\circ - (V - v);$$

therefore

$$r = \frac{A(e^2 - 1)}{1 - e \cos (V - v)}.$$

The value of  $V$  is found by the condition that when  $r = r_0$ ,  $v = 0$ . Then

$$r_0 = \frac{A(e^2 - 1)}{1 - e \cos V},$$

whence

$$\tan V = \frac{\sin \theta_0 \cos \theta_0}{\alpha a r_0 - \sin^2 \theta_0};$$

and putting, for the sake of brevity,

$$C = \frac{e}{1 - e \cos V} = \frac{\alpha a r_0 - \sin^2 \theta_0}{\sin^2 \theta_0 \cos V},$$

we shall find

$$r = \frac{r_0}{1 + C \cos V - C \cos (V - v)}$$

for the polar equation of the luminous trajectory, by which we can know the radius-vector and its inclination to the vertical line for any point of the trajectory.

In order to have the astronomical refraction corresponding to a given zenith-distance, it is necessary first to derive from this latter formula the angle at the centre corresponding to the radius-vector

$$r = r_0 + \frac{1}{a}$$

of the limiting layer of the atmosphere; afterwards to calculate the angle  $v'$  made by this radius with the last element of the luminous trajectory, by the formula

$$\sin v' = \frac{r_0 \sin \theta_0}{r} \sqrt{1 + 4k\rho_0}.$$

That done, the refraction  $R$  will be given, as may be easily seen, by the expression

$$R = v + v' - \theta_0.$$

Applying these formulæ to the calculation of the horizontal

refraction, we shall have

$$\begin{aligned}\theta_0 &= 90^\circ, \\ \cos v &= 1 - \frac{1}{(1 - ar_0)(1 + ar_0)}, \\ \sin v' &= \frac{ar_0}{1 + ar_0} \sqrt{1 + 4k\rho_0}.\end{aligned}$$

Taking

$$r_0 = 20,888,782 \text{ feet,}$$

$$a = 0.00002276,$$

$$4k\rho_0 = 0.00058856,$$

we get

$$v = 4^\circ 0' 12'', \quad v' = 86^\circ 33' 25'',$$

whence

$$R = 33' 37'',$$

a very satisfactory value. It is often repeated that the refraction computed on the hypothesis of uniformly decreasing density is less than the truth. But I may observe that this happens only when the rate of the decrease of density is taken too small. All works which treat of astronomical refraction on the assumption of an equable decrease of density take the height of the atmosphere at double that of a homogeneous atmosphere. Now although this height is that derived from the condition that the pressure and density are reduced to zero simultaneously at the upper limit of the atmosphere, yet it does not correspond to the real decrease of density observed in the inferior strata. The consideration of the higher strata of the atmosphere, which have no sensible effect on refraction, is of little importance to us. What we want is the rate of decrease of the density in the inferior portion of the atmosphere; and this experience shows us to be larger than that derived from an atmosphere of double the height of a homogeneous atmosphere.

For the purpose of the measurement of heights, we derive easily, from the preceding expression of the radius-vector, that

$$\frac{r_0}{r} (r - r_0) = x = 2r_0 C \sin \frac{v}{2} \sin \left( V - \frac{v}{2} \right),$$

which, added to the expression

$$r - r_0 = x + \frac{x^2}{r_0 - x},$$

will give the difference of level of two stations, when the zenith-distance  $\theta_0$  at one, and the angle  $v$  at the centre of the earth, between the radii drawn through the stations, are known.

The radius  $r$  being thus determined, the difference between



the apparent zenith-distance  $\theta_0$  and the true one, or the terrestrial refraction  $\delta$ , will be easily found by the resolution of the triangle made by the radii  $r_0, r$  and the rectilinear chord. This triangle gives

$$r_0 \sin (\theta_0 + \delta) = r \sin (\theta_0 + \delta - v),$$

whence

$$\cot (\theta_0 + \delta) = \frac{x}{r_0 \sin v} - \tan \frac{v}{2}.$$

Biot came to the same formulæ by a different way in his *Mémoire sur la Mesure Théorique et Expérimentale de la Réfraction Terrestre* (1838). The reader is referred to that paper, which contains likewise the formulæ for a law of density expressed by the first and second power of the height. Biot's three memoirs, *Mémoire sur la vraie Constitution de l'Atmosphère Terrestre déduite de l'expérience* (1838), *Mémoire sur les Réfractions Astronomiques* (1836), *Mémoire sur la Réfraction Terrestre* (1838), which will be found in the *Connaissance des Temps*, are the most complete writings that have hitherto appeared on the subject. But the observations employed therein, derived from Gay-Lussac's celebrated balloon-ascent, are contradicted by the latest experiments. In fact the latter eminent physicist was led to the conclusion that the temperature varied less, for a given change of elevation, near the earth than in the higher region. After Mr. Glaisher's experiments this opinion is no longer tenable.

In respect to the determination of the constants  $\alpha$  and  $a$ , I may make the following remarks. As we must admit that the state of the atmosphere, especially in the inferior strata, is continually changing, the parameters  $\alpha$  and  $a$  entering into the above formulæ are to be in each particular case determined by means of direct meteorological observations made at the moment in which the value of the refraction is wanted. The value of  $\alpha$  will be given by the barometer and thermometer of the observatory. The value of  $a$  will depend on these data, and will require besides the knowledge of the density of the air at a point sufficiently elevated in the atmosphere.

Supposing thus known by help of direct meteorological observations the densities  $\rho_0, \rho$  of the air at two stations situated at different heights, the value of  $a$  will be given by the formula

$$a = \frac{g_0}{2\rho_0} \left( \frac{\rho_0^2 - \rho^2}{p_0 - p} \right),$$

which is derived from the barometrical formula found above.

The densities  $\rho_0, \rho$  should be calculated by means of the total pressures  $p_0, p$ , as if the air were dry, because, as we said before, moist air refracts light, sensibly, as dry air under the same pressure

and at the same temperature. Wherefore

$$a = \frac{g_0}{2ct_0} \left[ \frac{1 - \left( \frac{pt_0}{p_0t} \right)^2}{1 - \frac{p}{p_0}} \right].$$

It is to be remarked that the method by which terrestrial refraction is usually calculated implies a decrease of density which agrees tolerably well with that resulting from Mr. Glaisher's observations. To show this, let us recall the expression

$$d\theta = - \frac{2k \left( \frac{d\rho}{dr} \right)}{1 + 4k\rho} r dv$$

of the element  $d\theta$  of the total refraction\*.

In geodesy it is assumed that the refraction is proportional to the angle at the earth's centre, or that we have

$$\Delta\theta = \theta - \theta_0 = Mv,$$

M being a constant.  $\Delta\theta$  is the angle contained between the tangents to the extremities of the arc intercepted between the object and the observer's eye, and is equal to the sum of the angles which the said tangents make with the chord of the arc. As the curvature of the luminous trajectory will vary but little in a small extent, the two angles are considered as equal; so that  $\frac{\Delta\theta}{2}$  will be the refraction at each station. The amount of terrestrial refraction is then nearly

$$\frac{\Delta\theta}{2} = \frac{M}{2} v.$$

It is generally assumed that

$$\frac{M}{2} = \frac{1}{12}.$$

Let us now inquire what law of decrease of density in the atmosphere this manner of operating presupposes.

In order that we may have

$$-2k \frac{\left( \frac{d\rho}{dr} \right) r}{1 + 4k\rho} = M,$$

it is necessary that

$$- \frac{2k d\rho}{1 + 4k\rho} = M \frac{dr}{r}.$$

By integrating from  $\rho_0$  and  $r_0$  to  $\rho$  and  $r$ , we shall get

\* Laplace, *Mécanique Céleste*, vol. iv. p. 277.

$$\frac{1}{2} \log \left( \frac{1+4k\rho}{1+4k\rho_0} \right) = M \log \left( \frac{r_0}{r} \right),$$

or

$$\frac{1+4k\rho}{1+4k\rho_0} = \left( \frac{r_0}{r} \right)^{2M},$$

which expresses the relation between the density of the air and the altitude involved in the hypothesis on which is grounded the method used in computing terrestrial refraction.

We derive from it, putting, as before,

$$\alpha = \frac{2k\rho_0}{1+4k\rho_0},$$

$$\frac{\rho}{\rho_0} = 1 - \frac{1}{2\alpha} \left[ 1 - \left( \frac{r_0}{r} \right)^{2M} \right].$$

Substituting for  $\frac{r_0}{r}$  its value,

$$\frac{r_0}{r} = 1 - \frac{x}{r_0},$$

and expanding, we shall find

$$\frac{\rho}{\rho_0} = 1 - \frac{M}{\alpha} \cdot \frac{x}{r_0},$$

neglecting the higher powers of  $\frac{x}{r_0}$ , which is a very small quantity.

Assuming

$\alpha = 0.00029$  (at the temperature of melting ice, and 0<sup>m</sup>.76 pressure),

$r_0 = 20,888,780$  feet,

$M = \frac{1}{6}$ ,

we get

$$\frac{\rho}{\rho_0} = 1 - (0.0000275)x.$$

Instead of the coefficient 0.0000275 we had found 0.00002276. The coefficient of terrestrial refraction corresponding to the latter value of  $a$  comes out 0.0689, or  $\frac{1}{14.5}$  nearly\*.

\* The following are the coefficients of refraction obtained experimentally:—

Bessel, from his operations in Prussia . . . . . 0.0685

Gauss, from his observations . . . . . 0.0653

Corabœuf . . . . . 0.0642

Struve . . . . . 0.0618

Mean derived from the survey in France . . . . . 0.0665

Mean derived from the survey in } 0.0809 for rays crossing the sea.

England. . . . . } 0.0750 for rays not crossing the sea.

(Ordnance Survey, London, 1858, pp. 512, 550.)

The coefficient  $\frac{M}{2}$  of terrestrial refraction is supposed usually in geodesy to have always the same numerical value at all places and at all times. This is obviously inexact\*. I should think it preferable to determine it by the observed decrease of density in each particular case. Having, by direct meteorological observations, the densities at two stations at different levels, we can calculate  $a$  by the formula above-mentioned,

$$a = \frac{g_0}{2\rho_0} \left( \frac{\rho_0^2 - \rho^2}{\rho_0 - \rho} \right),$$

and then we shall have for the coefficient of refraction

$$\frac{M}{2} = \frac{aar_0}{2}.$$

It is not to be forgotten that the densities  $\rho_0$  and  $\rho$  should be calculated as if the air were dry.

I shall close this long article by recapitulating the formulæ to be used for the measurement of heights, and applying them to a numerical example.

#### *Barometrical Formula.*

$$x = \frac{2}{g_0} \left( \frac{p_0 - p}{\rho_0 + \rho} \right),$$

$$\rho = \frac{p - \frac{3}{8} \varpi}{ct},$$

$$c = (29 \cdot 172) \text{ G in metrical measure,}$$

$$c = (53 \cdot 173) \text{ G in English measure,}$$

$$x = \frac{r_0(r - r_0)}{r},$$

$p_0, p$  atmospheric pressures at the lower and upper stations respectively.

\* It has been found that the amount of terrestrial refraction undergoes an hourly variation. A series of observations for determining the hourly variations was carried out by M. Hossard, at Angoulême, during the months of May and June 1844. The observations were made half-hourly, from daybreak till twilight; and the object observed was at a distance of about thirteen miles. The conclusions to which they led are the following:—The refraction is greatest about daybreak; from 5 or 6 A.M. until 8 A.M. it diminishes very rapidly; from 8 A.M. until 10 A.M. the diminution is slow; from 10 A.M. to 4 P.M. the refraction is nearly constant; from 4 P.M. the refraction commences to increase.



$\rho_0, \rho$  densities of the air mixed with aqueous vapour at the same respective stations.

$\varpi_0, \varpi$  pressures of the vapour contained in the atmosphere at the same stations.

$t_0, t$  absolute temperatures at the same stations, the absolute zero being  $274^\circ$  C., or  $493^\circ\cdot 2$  F. below the melting-point of ice.

$g_0$  gravity at lower station. At  $45^\circ$  of latitude and at the sea-level  $G = 9^m\cdot 80604 = 32\cdot 172$  feet (Bessel).

$r_0, r$  distances of the stations from the earth's centre.

*Example.* Measurement of Monviso, August 12th,  $2^h 30^m$  P.M. 1863, taking Verzuolo (425 metres) as the lower station:—

	Verzuolo.	Monviso.
Barometer reduced to the } freezing-point . . . }	0 <sup>m</sup> ·7239	0 <sup>m</sup> ·4836
Thermometer in open air . . .	$27^\circ\cdot 15$ C.	$6^\circ$ C.
Thermometer with wet bulb . . .	$21^\circ\cdot 3$	$6^\circ*$

Therefore

$$p_0 = 0^m\cdot 7239 \qquad p = 0^m\cdot 4836$$

$$t_0 = 301^\circ\cdot 15 \qquad t = 280^\circ \text{ C.}$$

$$\varpi_0 = 0^m\cdot 01535 \qquad \varpi = 0^m\cdot 007$$

Monviso above Verzuolo  $x = 3417\cdot 3$  metres.

Verzuolo above the sea . 425·0 „

Monviso above the sea . 3842·3 „

Taking into account the diminution of gravity in receding from the centre of the earth, the difference of level between Monviso and Verzuolo comes out 3426·7 metres, and the altitude of that peak above the sea 3851·7 metres.

Delcros's barometrical Tables, founded on Laplace's formula, give 3860·7 metres. It must be remarked that, if this result is only 9 metres in excess of that deduced from my formula, that is due to the use of the coefficient 18336 therewith made, in place of 18405, resulting from the more accurate determination of the relative weight of the air and mercury by M. Regnault, which should be used, and which would bring the difference to 22 metres.

#### *Geodetical Formula.*

$$x = 2r_0 C \sin \frac{v}{2} \sin \left( V - \frac{v}{2} \right),$$

$$r - r_0 = x + \frac{x^2}{r_0 - x},$$

\* The temperature of the thermometer with wet bulb was not observed on the top of Monviso; but as we were in the mist, the difference of wet and dry bulb must have been very small.

where

$$\tan V = \frac{\sin \theta_0 \cos \theta_0}{\alpha a r_0 - \sin^2 \theta_0},$$

$$C = \frac{\alpha a r_0 - \sin^2 \theta_0}{\sin^2 \theta_0 \cos V},$$

$$\alpha = \frac{2k\rho_0}{1 + 4k\rho_0},$$

$$a = \frac{g_0}{2\rho_0} \left( \frac{\rho_0^2 - \rho^2}{\rho_0 - \rho} \right),$$

$$4k\rho_0 = 4k[\rho] \frac{274g_0 H}{(0^m.76) GT},$$

$$4k[\rho] = 0.00058856 \text{ at } 0^\circ \text{ C.},$$

$$0^m.76 \text{ pressure, } 45^\circ \text{ lat. (Biot.)}$$

$\theta_0$  zenith-distance of the signal.

$r_0, r$  distance of the stations from the earth's centre.

$v$  angle between the radii  $r_0, r$ .

Earth's radius at lat.  $45^\circ$   $6,366,786^m = 20,888,782$  feet. (Arago.)

*Example.* Measurement of Monviso above Turin. Data extracted from *Mesure d'un Arc du Parallèle Moyen*, vol. ii. p. 384.

Zenith-distance of Monviso seen from Turin .  $\theta_0 = 87^\circ 8' 13''$

Distance from Turin to Monviso . . . .  $v = 0^\circ 34' 53''$

Earth's radius at Turin . . . . .  $r_0 = 6,366,273^m$

Turin { Barometer . . . . .  $0^m.7464$   
Thermometer in open air . .  $10^\circ.6 \text{ C.}$

In the absence of simultaneous meteorological observations at the top of Monviso, I shall determine, merely for the sake of illustration, the constant  $a$ , viz. the rate of decrease of the density of the air, by means of the simultaneous observations at Verzuolo and Monviso above-mentioned. From them we deduce

$$a = 0.000082941.$$

With these data our formulæ give

$$4k\rho_0 = 0.00055455, \quad \alpha = 0.00027712,$$

$$\alpha a r_0 = 0.14633,$$

$$V = -3^\circ 21' 15'',$$

$$C = -0.85477,$$

$$x = 3510^m.4.$$

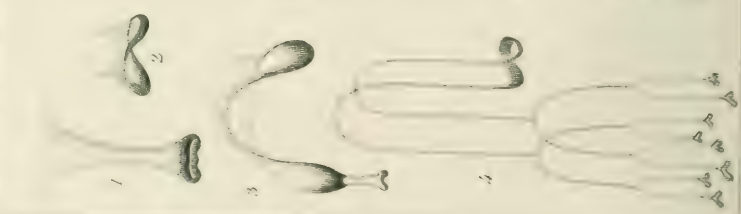
Monviso above Turin  $r - r_0 = 3512.3$  metres.

Turin altitude . . . .  $273$  „

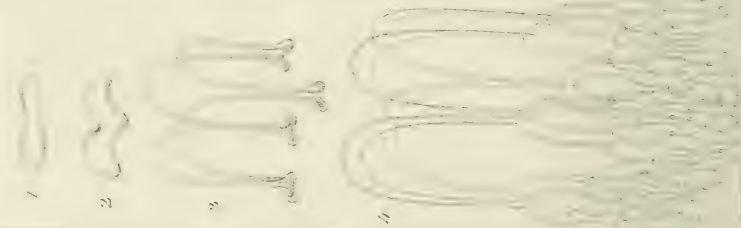
Monviso above the sea . .  $3785.3$  „



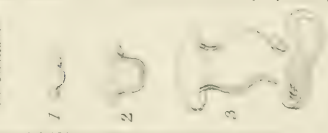
*Coined in Liquid Water*



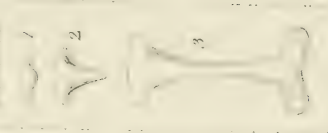
*Coined in Air under a Spout*



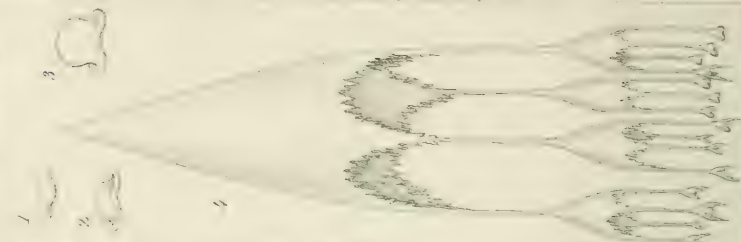
*Coined in Air under a Spout*



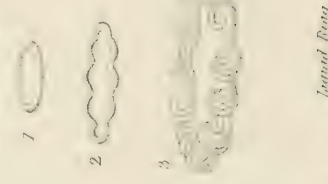
*Coined in Air under a Spout*



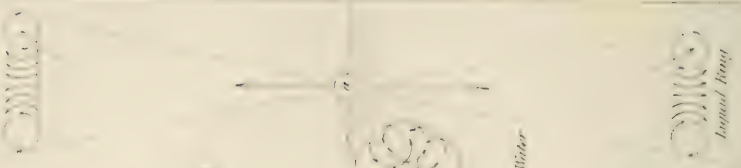
*Coined in Air under a Spout*



*Coined in Air under a Spout*



*Coined in Air under a Spout*





The value of the refraction corresponding to this difference of level comes out  $2' 33''$ , viz. about  $\frac{1}{11}$ th of the angle at the centre of the earth.

In the *Mesure d'un Arc du Parallèle Moyen*, the altitude arrived at is  $3798^m.2$ , which corresponds to a refraction of  $1' 46''$ , or  $\frac{1}{20}$ th nearly of the angle at the centre. It is presumable that the zenith-distance there given is rather too great; for the altitude of Monviso is 3836 metres according to Corabœuf, and 3840 metres according to the *Etat Major Piémontais*. In reality the distance between Turin and Monviso is too large for the base of a hypsometrical triangle: an error of  $1'$  in the zenith-distance produces an error of about 19 metres in the altitude. Even neglecting the refraction, the altitude of Monviso with these data would only come out 3833.4 metres.

Turin, April 1864.

LXV. *On a new Variety of the Cohesion-Figures of Liquids.* By CHARLES TOMLINSON, *Lecturer on Physical Science, King's College School, London* \*.

[With a Plate.]

IN the Philosophical Magazine for October 1861 and March 1862, I have given a number of details respecting the Cohesion-Figures of Liquids. The results of a more mature experience in this subject were laid before the Pharmaceutical Society in February last, and are recorded in the Pharmaceutical Journal for March and April†. The attention of persons interested in the oil trade was also invited to the subject in a paper read before the Society of Arts in March last, and printed in the Society's Journal for the 4th of that month‡. My object in bringing the subject before these Societies was to endeavour to enlist the sympathy of working chemists in carrying out the practical application of this test. I had already established the principle that liquids, under certain conditions, form characteristic figures, which seemed likely to be useful in qualitative analysis, if only as a rough and ready mode of identification, although in some cases it seemed to admit of a rigour and delicacy which could not be claimed for existing tests. What was wanted for the further prosecution of the inquiry was, (1) the collection of a large number of specimens, commercially

\* Communicated by the Author.

† "On the Verification of Castor Oil and Balsam of Copaiba by means of their Cohesion-Figures."

‡ "On the Verification of Olive Oil by means of its Cohesion-Figure." I may here remark that the figure of olive oil which accompanies my first paper in the Philosophical Magazine was not obtained from a pure specimen, but from one adulterated with poppy-seed oil.

if not chemically pure, of the same liquid, produced in various countries and under different conditions; (2) the examination of such specimens, with the view of obtaining standard cohesion-figures; and (3) the effect of admixtures of different liquids in various proportions on such figures. I had already taken considerable pains in ascertaining the figures of a number of liquids; and not being able to bestow the time required for further investigation, even were I able to command the specimens; it seemed best to lay the subject before such societies as those named, many of the members and students of which had remarkable facilities for carrying it out; and besides this, as the journals of those societies circulate largely among persons interested in the possession of a ready test for determining the purity of commercial oils and other liquids, the subject was likely to attract the attention of practical men.

Most of the figures described in my papers are produced by placing a drop of liquid on the surface of chemically clean water in a chemically clean glass; and the phenomena are confined to the surface. If the drop be placed on any other surface than water, such as sulphuric acid, acetic acid, alcohol, benzole, &c., the figures vary, because the adhesive force, the diffusibility, &c. vary. If the drop be considerably heavier than the liquid which forms the adhesive surface, it will sink and diffuse more or less through the body of the liquid, producing a new set of figures, of great variety and beauty, which I propose to consider in the present communication.

A good typical example of this class of cohesion-figures by submersion (not *on the surface*, as in the former experiments) is furnished by a solution of cochineal in distilled water. Three solutions were made, one with 15 grains, another with 20 grains, and a third with 40 grains of cochineal, in one fluid-ounce of distilled water. The solutions were filtered, and, after each had run through, half an ounce of water was poured on the filter; and three other solutions were thus obtained, and were kept apart. A single drop of one of these solutions was delivered from a pipette to the surface of water contained in a cylindrical glass. A small quantity of a solution of alum or of oxalate of ammonia had been previously added to the water for the purpose of throwing down the lime; and even when distilled water was used, the alum adds to the persistency and beauty of the figure by its tendency to form a *lake* with the colouring matter.

As soon as the drop of cochineal solution falls beneath the surface, it expands into a ring, sinks a short distance further, and then becomes poised (see Plate IV. Cochineal, No. 1). The more diffusive portions of the colouring matter stream upwards

in a faint flame-like circular cloud ; the denser portions accumulate at two opposite points of the ring, which is thus, at its thinner portions, bent upwards (No. 2) and then drawn downwards into graceful festooned lines by the heavier portions, which form separate rings smaller than the parent ring (see No. 3). These rings in like manner descend ; the denser portions of colouring matter accumulate at the two opposite points  $90^\circ$  from the points of attachment to the festooned lines ; each small ring is in this case also bent upwards, while it drops two other rings, which in their turn go through the same series of changes, until the figure becomes almost too complicated to follow (see No. 4). This is a very common result with a moderately weak solution of cochineal ; with a stronger solution, the figure undergoes some beautiful modifications. The heavier portions of the colouring matter collect not at two, but at four, six, or even seven or eight points of the ring\*—bending it upwards in as many curved lines, and letting drop as many rings, each of which becomes the seat of manufacture of two rings ; and in this polypus method of division and subdivision the colouring matter is diffused through the solution.

The colour of the figures may, of course, be altered by adding to the water a few drops of a solution of nitrate of copper, of a salt of tin, of ammonia, &c.† If too much be added, the rings will not go through their changes, but subside to the bottom of the vessel, and remain for hours as rings. If the drops of cochineal be allowed to fall slowly into the water through a small funnel containing a filter, rings only will be formed ; and these being nearly equidistant, and expanding as they fall, call to mind the beautiful rings formed by bubbles of phosphuretted hydrogen bursting in air. I have noticed more than once, that as the drops fell slowly through the funnel, and the solution in the filter became denser, a ring would overtake its predecessor, shoot through it, and expand beneath it without disturbing it. But the best method of exhibiting these figures is that first described—namely, by allowing a single drop to fall into the water, and to go through its various changes before a second

\* See the figures Nos. 1, 2, and 3 of cochineal in water containing a little oxalate of ammonia ; but the effect is not confined to the presence of this salt.

† In a glass cylinder 11 inches high and 3 inches in diameter, into which about half an ounce of solution of ammonia had been poured before filling it up with New River water, figures of great symmetry and beauty were produced sufficiently slow in their development to admit of being studied. In this instance, as in some others to be noticed presently, the curved festoons were connected by a very delicate membrane. If the cylinder be kept covered when not in use, the solution will act for a week or more without being renewed.



drop is allowed to fall; and these changes may occupy a minute or two.

The cochineal figure, with its dropping rings and festoons, is exhibited in its most complicated form by some of the essential oils in a column of alcohol, or some other solvent of them. For example, rather more than three fluid-ounces of methylated spirits of wine were poured into a cylindrical glass 6 inches high and  $1\frac{1}{4}$  inch in diameter, and on the surface of this was deposited, from the end of a glass rod, a single drop of oil of lavender. The drop sank beneath the surface, expanded into a ring No. 1\*, passed through the changes Nos. 2 and 3, and then burst like a rocket into a multitude of festoons and small rings. An attempt is made to represent the lavender figure in the Plate, in which will be seen (1) the ring, (2) the same bent down at four equidistant points, from which proceed (3) festooned lines and other systems of rings, and from these last we get (4) a multitude of small rings and festoons. The figure formed by oil of cubeb is even more remarkable. The drop formed a large ring, which split into two or three other rings, each of which broke up into lines and small rings, and a multitude of globules descending and dragging trails after them. Oil of cinnamon formed a good ring, which descended before it broke up, leaving a ring of more attenuated matter behind it; just as in the cohesion-figure of this oil on the surface of water a more diffusive oil shoots out beyond the usual figure and forms a fringe to it. The texture of the rings is like that of molten glass or sugar; indeed the term *saccharine* would express the appearance of the rings and festoons in spirits of wine. In a few cases the rings and lines were chalky, as if the elaiopten were dissolved first and the stearopten were left to do the work of the figure. This effect is produced by oil of peppermint, but is much more marked in columns of other liquids afterwards employed. The rings and lines formed by benzole in spirits of wine are very glassy and wavy. The oils of turpentine and juniper form each a double convex lens, or a very flat spheroid, which descends rapidly and remains at the bottom. The fixed oils also form very flat spheroids.

When a column of benzole was used, many of the essential oils produced the rings and festoons, but often of an opaque white, as if the stearopten alone formed the figure. In some cases the first ring would be glassy and transparent, and then burst into a figure that was chalky or milky. The oils of lavender, coriander, cinnamon, cloves, bergamot, and some others formed smoky rings and festoons. The oils of origanum, rosemary, and some others formed transparent lines and rings.

\* See oil of lavender, &c., in spirit, in the Plate.



The oil of bitter almonds formed a chalky ring, which descended through the column of liquid, expanding as it descended, and rotating rapidly about its curved axis inwards, *i. e.* towards the vertical axis, an effect that will be referred to more particularly hereafter. The oil of rue sank some way in the liquid, flashed into rings which expanded upwards. Oil of cubebs, which was so beautiful in spirits of wine, had no action in benzole. Oil of wintergreen formed a fine glassy-looking ring, or rather disk, with a thickened edge; the disk became cup-shaped, then broke up into rings and festoons of some breadth, which were dotted over with a pattern. A drop of camphorated spirit descends like a hollow cup, enclosing globules of spirit; and when it breaks up into rings and festoons, the latter are studded with minute shells or globules of camphor, producing a pretty effect. Turpentine forms a good figure; and an inferior oil of lavender, probably adulterated with turpentine, behaved somewhat like it, very different from the purer oil. The fixed oils form disks which descend with waving edges, as in the case of olive oil, or cup-shaped vessels, which become drawn out in descending, as shown in the figure which refers to croton or colza oil. The drop forms first a well-balanced dish (No. 1), the bottom of which descends (No. 2) and draws with it portions of the side (No. 3).

The effect last described is modified when a drop of croton oil is allowed to descend through a column of paraffin oil. The drop is drawn out into the figures (Nos. 1, 2, and 3) represented in the Plate, as if the bottom fell out and the sides collapsed. But the most remarkable effect is with a drop of fousel oil in a column of paraffin oil. The drop first forms a dish (No. 1) bagging downwards, and this almost immediately expands upwards (No. 2), swells out into a dome or cone (No. 3), the ring expanding all the time; the point of the cone remains fixed in the liquid, while the lower edge becomes arched at four equidistant points, the edges of the arches beautifully fringed, and lets fall lines with drops at the ends, which form separate cones, each of which becomes arched, and lets down other lines with drops. In this way a figure (No. 4) is produced of great beauty, and with an architectural kind of effect which is very striking. The duration is also considerable. The texture of the figure is gauze-like and delicate. Absinthe and neroli also form beautiful gauze-like figures in paraffin oil. Oil of cubebs lets drop two or three horizontal rings, not connected by festoons. Oil of cloves produces a similar result. Kreosote forms delicate chalky lines and globules. Oil of bitter almonds forms a convex lens; camphorated spirit is spheroidal; lavender breaks up into rings and festoons, but with considerable differences in different specimens, depending on the purity.

When a column of turpentine is used, a drop of oil of cubebs descending in it, opens into widely expanding transparent rings, which break up into small rings and festoons. A drop of kreosote also separates into two or three transparent rings, which break up into smaller ones, connected by fine lines of a peculiar dull opake white colour. A drop of carbolic acid presents similar phenomena, only the lines are glassy and transparent throughout. A drop of camphorated spirit forms a veil, from which descends a multitude of spirit-globules, and the camphor disappears by solution. The oils of cloves and cinnamon also make good figures.

In pyroligneous ether, the oils of rosemary and lavender, as also kreosote, formed rings and festoons; the oil of verbena the same, only they were chalky and persistent. Several essential oils formed beautiful rolling rings before splitting up, such as the oils of cloves, cajuput, chamomile, carraway, as also benzole and carbolic acid. Water flashed into a ring and descended, expanding and rolling on its curved axis, after the manner of phosphide of hydrogen. A drop of hydrochloric acid and of ammonia did the same. The oils of bergamot, patchouli, origanum, and camphorated spirit formed figures after the type of the cochineal solution in water, only instead of dropping a third set of rings from the second set, oblate spheroids were dropped. The oil of juniper descended as a sphere, as did also turpentine, only the latter, having reached the bottom, ascended slowly like a balloon in air. Olive and croton oils formed flattened spheroids, the latter oil yielding much flatter ones than the former.

In a column of ether, if the drops did not immediately enter into solution, they formed very beautiful rolling rings. The hydrocarbons of mint and thyme were very fine; benzole, turpentine, and cajuput were also particularly good; a drop of turpentine descends rapidly as a waving, trembling, rotating ring, and disappears about two-thirds down. An attempt has been made to represent some of these rings in the Plate. A drop of the bright red tincture of cochineal forms good rolling rings in ether, but the colour changes to purple even before the drop reaches the surface of the ether.

Having obtained these rolling rings in ether, I had no difficulty in producing them in a tall column of water on a larger scale, by allowing drops of a saturated solution of common salt to fall on the surface from a dropping tube. A strong solution of nitrate of copper also answered very well.

I was talking over the subject of these rolling rings with my friend Professor Miller, and expressing my opinion that they explained perfectly the rolling motion of the phosphuretted hy-

drogen rings in air, which I thought had not been correctly stated in the books, when he informed me that Professor Rogers had, a few years ago, sent him a copy of a paper on this part of my subject. I lost no time in reading this paper in the *American Journal of Science* for September 1858\*, and was delighted at the ingenious mode in which the subject had been treated. An apparatus was constructed, from which rings of the vapour of sal-ammoniac could be shot out into the air at pleasure, while another apparatus let fall into water rings of water charged with chromate of lead, or carbonate of lead, sulphate of lead, sulphate of baryta, cobalt blue, or a dilute solution of sulphate of indigo. Professor Rogers noticed that the rotation of the ring in air "has always one direction, the inner circumference being carried forwards, or, in the ordinary mode of the experiment, upwards, and the outer in the reverse direction," and that "the rotation is due to the combined agency of the outward impulse and the resistance which the sides of the issuing mass encounter from the edge of the opening, and from the air into which it is impelled. The former of these forces, due to the tension propagated through the reservoir, must to some extent operate in diverging directions, while the resistance acts in nearly opposite ones. Thus at the outset there would be produced a reversion or curling of the issuing cloud around the aperture, which, as the action continued, would be developed into the spreading volutes before described, and at length into the perfect and rapidly revolving ring. The dilation of the ring in its ascent would seem to be the natural result of the divergent character of the impulse impressed upon the air as it passes from the orifice." In the case of the phosphuretted hydrogen ring, Professor Rogers insists upon "the chemical action of great intensity that sets in between the included gas and the air."

I admire Professor Rogers's paper so much, that I do not like to criticize it; but I cannot help thinking that his explanations would be simplified had he taken the case of a ring of smoke ascending in air, or of a drop of a solution descending in water, without embarrassing the subject with conditions depending (1) on the form of the orifice, (2) the tension of the enclosed gas, (3) the chemical action, and (4) the force with which the liquid is projected. Take the case of a liquid ring. The forces are (1) *diffusion*, which forms the ring; (2) *gravity*, which causes it to sink. The resistance is *friction*, retarding (1) the descent of the ring, (2) its diffusion. In the case of a ring of smoke, the forces are precisely the same, only gravity causes it to ascend, and friction retards the ascent. I

\* "On the Formation of Rotating Rings by Air and Liquids under certain conditions of Discharge," by Prof. W. B. Rogers, p. 246.



have attempted to combine both cases in the figure, where the globule *a* may be either a bubble of  $\text{PH}_3$  about to burst and project a ring of smoke into the air, or it may be a drop of liquid about to descend in water, benzole, ether, &c., as in the previous experiments. The ring of smoke, or the liquid ring, acts as if rolling up or down the inside of a hollow cone, and the direction of rotation of the particles will be found according to this view. In both cases the tendency of the ring is constantly to enlarge by diffusion, and the rate at which it does enlarge is regulated by the resistance of the liquid column or of the air. The resistance of the liquid column is much greater for the liquid ring than of the air for the smoke ring; and hence the liquid rings do not expand much, while the smoke rings expand greatly. But the resistance, whatever its amount, must clearly be applied to the outer surface of the ring; or the ring may be said to bear on the surrounding medium by its outer surface, which would be equivalent to its rolling up the inner surface of a hollow cone. In the figure, the straight vertical arrows show the motion of the ring up or down, the oblique arrows give the direction of the resultant of the forces acting on the ring, and the direction of the resistance of the medium to this resultant, while the curved arrows show the direction in which rotation must occur according as the general direction of the movement of the rings is upwards or downwards.

The separate ring of arrows is nearly the same as that given in a little book of mine published by the Christian Knowledge Society in 1848, and intended to illustrate the formation of a ring of smoke as projected from a factory chimney (here representing a liquid ring or an air ring, according to the position in which the plate is held). It explains the appearance in the liquid rings of two Ionic-like volutes on either side of the ring, and produced by the perspective of a number of rings seen through, or nearly through, each other, while at the front and back the edges of single rings only are seen.

King's College, London,  
April 1864.

LXVI. *On the Refractive Index of the Metals*. By G. QUINCKE\*.

**I**N a former memoir "On the Optical Properties of the Metals," I have called attention to the fact that the refractive index of the metals, especially of silver and gold, can be less than 1†. This followed from the application of Cauchy and Eisenlohr's theory to the constants of the elliptical polariza-

\* Translated by Professor Wanklyn from Poggendorff's *Annalen*, vol. cxix. part 4 (1863), p. 599.

† See *Phil. Mag.* vol. xxvii. p. 161.



tion of the light reflected from these metals, as well as directly from my researches on the displacement of the interference-bands by means of thin metallic plates.

At the same time, however, the theory of elliptical polarization, as developed by Cauchy, Beer, and F. Eisenlohr, leads to this, that the refractive index of the metals, just like the extinction-coefficient, is a function of the angle of incidence; and indeed it follows that (see *Phil. Mag.* vol. xxvii. p. 169)

$$\left. \begin{aligned} \nu^2 &= n^2 + \sin^2 I, \\ \gamma^2 &= g^2 + \sin^2 I, \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$\nu$  and  $\gamma$  being the refractive and extinction-coefficients for the angle of incidence  $I$ ,  $n$  and  $g$  being similarly related to the angle of incidence  $0^\circ$ .

The correctness of the first equation (4) may be shown by using the interference-apparatus employed by me (described in my former memoir (see vol. xxvii. p. 169), by observing the displacement of the fringes of interference whilst the metallic plate lying in the path of one of the interference-pencils of rays is gradually inclined so that the angle of incidence of the incident rays gradually increases.

As the inclination increases, the thickness of metal traversed by the ray will increase, and consequently the displacement produced thereby will also increase. On the other hand, however, the refractive index  $\nu$  will also be increased, and for a certain angle of incidence  $\eta$ , we shall have  $\nu=1$  when we have  $n<1$ . With an increasing angle of incidence, the displacement will accordingly increase up to a certain extent, and then again diminish. For a certain angle of incidence  $\eta$  the displacement will be 0, and then we have, from equation (4),

$$\left. \begin{aligned} 1 &= n^2 + \sin^2 \eta, \\ n &= \cos \eta. \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Assuming the correctness of the theory, we have thus a method of estimating  $n$  without knowing the thickness of the plate. In this it is supposed that the displacement of the bands of interference arising from the elliptical polarization of the light passing through the metal is 0.

If it be granted that the view propounded and supported by me on former occasions\* is correct, viz., that the rays polarized perpendicular to the plane of incidence, and of which the vibrations are perpendicular to the plane of incidence, suffer either the same or no alteration at all of phase under all angles of incidence, but that this is the case only with rays polarized parallel to the plane of incidence, then it is only necessary to

\* *Monatsbericht der Berliner Academie*, Dec. 18, 1862, and March 16, 1863.

observe the displacement of the bands of interference with a Nicol's prism, of which the principal section is parallel to the plane of incidence, so that only those rays reach the eye which are polarized perpendicular to the plane of incidence of the metal.

From the nature of the case, no high degree of accuracy is attainable in observations of this kind; for, on account of the great difference between the intensity of light which has passed through air and the intensity of that which has passed through metal, the interference-bands are only weak, and an error of 0.1 of the space between the fringes may be easily made. To this must be added that the rays which are polarized in a vertical plane, perpendicular to the plane of reflexion of the thick plane-parallel glasses of the interference-apparatus, have altogether only a low intensity. In the same way the rays polarized perpendicular to the plane of incidence of the metal are less weakened by the elliptical polarization of the light transmitted through the metallic plate, than the rays polarized parallel to the plane of incidence of the metal. In general, therefore, the observations on rays which are polarized parallel to the plane of incidence of the thick plane-parallel glasses, and perpendicular to the plane of incidence of the metal plate, deserve greater confidence on account of the greater intensity of the light; and thence, in order to alter the angle of incidence  $I$ , the metal plate must be turned about a horizontal axis. However, for the sake of control, observations by turning the metallic plate about a vertical axis were instituted, as well as for light polarized perpendicular to the plane of reflexion of the thick plane-parallel glasses.

The following Table gives the observations on a plate of blue-violet silver, which, according to Foucault's process\*, was formed and polished on a plane-parallel glass plate. The thickness was 0.000122 to 0.0001788 of a millimetre. The light falling perpendicularly, for  $I=0$  I observed a displacement of the interference-bands equal to  $-0.2$  or  $-0.3$  of the distance of the interference-bands in the spectrum in the neighbourhood of Fraunhofer's line F. The negative sign of the displacement indicates that  $n < 1$ . The first column contains the angle of incidence; the following contain the observed displacement in fractions of the distance of the fringes according as the light was polarized parallel ( $\parallel$ ), or perpendicular ( $\perp$ ) to the plane of incidence of the transparent metallic plate. For the second and third columns this plane of incidence was horizontal; for the fourth and fifth vertical. It is at the same time recorded whether the field of vision was light or dark.

\* Le Verrier, *Ann. de l'Observatoire Impériale*, vol. v. p. 179.

Plane of Incidence of the Metal.	Horizontal.		Vertical.	
	Field.	Light.	Dark.	Light.
	I.	$\perp$	$\perp$	$\perp$
0		-0.25	-0.25	-0.25
20		-0.35	-0.3	-0.3
30		-0.3	-0.3	-0.3
40		-0.4	-0.27	-0.2
50		-0.3	-0.2	-0.1
60		-0.2	-0.1	-0.1
70			-0.1	-0.

From this it appears that, as a rule, the displacement for light polarized parallel to the plane of incidence is about 0.1 of the distance of the fringes greater than for light polarized perpendicular to the plane of incidence,—or that, in accordance with my experiments with the Babinet's compensator, the rays polarized in the plane of incidence are before the others. These last rays polarized perpendicular to the plane of incidence undergo at  $I=70^\circ$  no more displacement; hence, according to equation (5),

$$\eta = 70^\circ, \cos \eta = n = 0.342.$$

It is here indeed supposed that the rays polarized perpendicular to the plane of incidence undergo no alteration in phase by traversing the metal plate; and this, according to other experiments of mine, seems to be not in all strictness true.

The refractive index  $n$  may likewise be calculated from the measured displacement  $\Delta$  of the interference-bands, the thickness  $D$  of the metallic plate being known. Let  $a$  be the distance between interference-bands in the spectrum,  $\lambda_m$  the length of the undulations belonging to the colour that we are dealing with in the metal,  $\lambda$  the same in air, then

$$\Delta = a \cdot D \left( \frac{1}{\lambda_m} - \frac{1}{\lambda} \right)^* ; \dots \dots \dots (7)$$

or, if the refractive index of the metal be used,

$$n = \frac{\lambda}{\lambda_m},$$

$$\Delta = a \cdot \frac{D}{\lambda} (n - 1) ; \dots \dots \dots (8)$$

whence follows

$$n = 1 + \frac{\lambda}{D} \cdot \frac{\Delta}{a} \dots \dots \dots (9)$$

\* See Neumann, "Law of the Double Refraction of Light in compressed Bodies," *Abhand. der Acad. der Wissenschaft zu Berlin*, 1841, vol. ii. p. 52.

$\frac{\Delta}{a}$  is the observed displacement of the interference-bands, measured in multiples of the distance between the fringes. This is positive when the light has a less velocity in the metal plate, negative when a greater velocity, than in air.

If the theory be correct, then the same value for  $n$  must be obtained from equations (5) and (9).

The displacement of the interference-bands in the spectrum of the apparatus employed was now observed near Fraunhofer's line F, so that

$$\lambda = 0.0005 \text{ millim.}$$

The displacement  $\frac{\Delta}{a}$  amounted to  $-0.25$  of the distance between the fringes at perpendicular incidence. At the place where the rays had passed through the silver, a bit of iodine was placed, in order to make an estimation of the thickness of the silver at this spot according to the method of Fizeau\*. There was formed a layer of iodide of silver, which (looked at perpendicularly) appeared yellow by transmitted, rust-brown by reflected light, and was bordered by three dark rings. This corresponds to a thickness of air equal to  $0.001652$  of a millimetre, with Newton's coloured rings. Putting  $\epsilon$  for the thickness of the corresponding layer of air in Newton's coloured rings of a similar tint to the iodide of silver, and, further,

Equivalent of Silver . . . . .	Ag = 107.9
Equivalent of Iodide of Silver . . .	IAg = 234.9
Thickness of the Iodide of Silver . .	$d_1 = 5.602$
Thickness of the Silver . . . . .	$d_2 = 10.55$
The Refractive Index of Iodide of Silver $n_1$ =	2.246,

then the thickness  $D$  of the film of silver out of which the iodide of silver was formed becomes

$$D = \frac{Ag}{IAg} \cdot \frac{d_1}{d_2} \frac{1}{n_1} \epsilon = 0.1086 \epsilon; \quad . \quad . \quad (10)$$

or, for this special case,

$$D = 0.1086 \cdot 0.001652 \text{ millim.} = 0.0001788 \text{ millim.}$$

Putting in equation (9) this value for  $\lambda$ ,  $D$ , and  $\frac{\Delta}{a}$ , then

$$n = 1 + \frac{0.0005}{0.0001788} \cdot (-0.25) = 0.323.$$

The correspondence of this value with that found by the other method is much closer than the accuracy of the experiments would lead us to expect.

\* *Comptes Rendus*, vol. lii. 1. 1861, p. 274.



A determination with another silver plate gave

$$D = 0.00005985 \text{ millim.}; \quad \frac{\Delta}{a} = -0.05;$$

and thence, by equation (9),

$$n = 0.6.$$

The difference between the two determinations 0.3 and 0.6 will not appear strange, since, as I have already remarked\*, much greater differences occur in the refractive index of the same metal; and a metallic plate has its refractive index altered by simple standing, so that it becomes even greater than 1.

For the rest it may be remarked that by the aid of the theory Eisenlohr† has deduced from Jamin's observations for the elliptical polarization of light of the colour of Fraunhofer's line F, which was reflected on a polished silver plate,

$$n = 0.4971,$$

a value which lies between those found by me.

Hence the result of the above-mentioned researches is, *that the refractive index of the metals is dependent upon the angle of incidence, and increases with an increasing angle of incidence.*

LXVII. *Note on the Nodal Curve of the Developable derived from the Quartic Equation*  $(a, b, c, d, e \chi t, 1)^4 = 0$ . By A. CAYLEY, Esq.‡

CONSIDERING the coefficients  $(a, b, c, d, e)$  as linear functions of the coordinates  $x, y, z, w$ , then the equation

$$\text{Disct. } (a, b, c, d, e \chi t, 1)^4 = 0,$$

or, as it may be written,

$$(ae - 4bd + 3c^2)^3 - 27(ace + 2bcd - ad^2 - b^2e - c^3)^2 = 0$$

represents, as is known, a developable surface or "torse," having for its edge of regression (or cuspidal curve) the sextic curve the equations whereof are

$$ae - 4bd + 3c^2 = 0,$$

$$ace + 2bcd - ad^2 - b^2e - c^3 = 0;$$

and for its nodal curve, a curve the equations whereof (equivalent to two independent relations between the coordinates) are

$$\frac{ac - b^2}{a} = \frac{ad - bc}{2b} = \frac{ae + 2bd - 3c^2}{6c} = \frac{be - cd}{2d} = \frac{ce - d^2}{e};$$

\* Phil. Mag. vol. xxvii. p. 172.

† Pogg. Ann. vol. civ. p. 375.

‡ Communicated by the Author.

or, as these may also be written,

$$\begin{aligned}
 a^2d - 3abc + 2b^3 &= 0, \\
 a^2e + 2abd - 9ac^2 + 6b^2c &= 0, \\
 abe - 3acd + 2b^2d &= 0, \\
 ad^2 - b^2e &= 0, \\
 ade - 3bce + 2bd^2 &= 0, \\
 ae^2 + 2bde - 9c^2e + 6cd^2 &= 0, \\
 be^2 - 3cde + 2d^3 &= 0;
 \end{aligned}$$

which curve is in fact an excubo-quartic,—viz. a quartic curve the partial intersection of a quadric surface, and a cubic surface, having in common two non-intersecting right lines. To show that this is so, I remark that the coefficients  $a, b, c, d, e$ , qua linear functions of the four coordinates, satisfy a linear equation which may be taken to be

$$a + b + c + d + e = 0.$$

This being so, the first form shows that the curve in question lies on the quadric surface

$$ac - b^2 + \frac{1}{2}(ad - bc) + \frac{1}{6}(ae + 2bd - 3c^2) + \frac{1}{2}be - cd + ce - d^2 = 0.$$

or, as this equation may also be written, substituting for  $c$ ,

$$\begin{aligned}
 &c \left( a - \frac{1}{2}b - \frac{1}{2}c - \frac{1}{2}d + e \right) \\
 &- b^2 + \frac{1}{2}ad + \frac{1}{6}(ae + 2bd) + \frac{1}{2}be - d^2 = 0.
 \end{aligned}$$

Substituting for  $c$  its value, this equation is

$$-(a + e + b + d) \frac{3}{2}(a + e) - b^2 + \frac{1}{2}ad + \frac{1}{6}(ae + 2bd) + \frac{1}{2}be - d^2 = 0,$$

or, what is the same thing,

$$9(a + e + b + d)(a + e) + 6(b^2 + d^2) - 3(ad + be) - (ae + 2bd) = 0.$$

Whence, finally, the equation of the quadric surface is

$$\begin{aligned}
 &9a^2 + 17ae + 9e^2 \\
 &+ 6b^2 - 2bd + 6d^2 \\
 &+ 9ab + 9de + 6ad + 6be = 0;
 \end{aligned}$$

and the curve lies also on the cubic surface

$$ad^2 - b^2e = 0.$$

It only remains to show that these surfaces have in common two right lines, and to find the equations of these lines.

The cubic surface is a skew surface or “scroll” such that the

equations of any generating line are  $d - \theta b = 0$ ,  $e - \theta^2 a = 0$ , where  $\theta$  is an arbitrary parameter. But considering the two lines

$$(d - \theta_1 b = 0, e - \theta_1^2 a = 0), \quad (d - \theta_2 b = 0, e - \theta_2^2 a = 0),$$

the general equation of the quadric surface through these two lines may be written

$$\begin{aligned} & A \quad (d - \theta_1 b) (d - \theta_2 b) \\ & + B \quad (e - \theta_1^2 a) (e - \theta_1^2 a) \\ & + C \quad (d - \theta_1 b) (e - \theta_2^2 a) + (d - \theta_2 b) (e - \theta_1^2 a) \\ & + \frac{D}{\theta_1 - \theta_2} \{ (d - \theta_1 b) (e - \theta_2^2 a) - (d - \theta_2 b) (e - \theta_1^2 a) \} = 0, \end{aligned}$$

or, expanding and reducing,

$$\begin{aligned} & A \{ d^2 - (\theta_1 + \theta_2) bd + \theta_1 \theta_2 b^2 \} \\ & + B \{ e^2 - (\theta_1^2 + \theta_2^2) ea + \theta_1^2 \theta_2^2 a^2 \} \\ & + C \{ 2de - (\theta_1^2 + \theta_2^2) ad - (\theta_1 + \theta_2) be + \theta_1 \theta_2 (\theta_1 + \theta_2) ab \} \\ & + D \{ (\theta_1 + \theta_2) ad - be - \theta_1 \theta_2 ab \} = 0, \end{aligned}$$

which, if  $\theta_1, \theta_2$  are the roots of the equation  $\theta^2 - \frac{1}{3}\theta + 1 = 0$ ,

and therefore  $\theta_1 + \theta_2 = \frac{1}{3}$ ,  $\theta_1 \theta_2 = 1$ , and  $\theta_1^2 + \theta_2^2 = -\frac{17}{9}$ , is

$$\begin{aligned} & A \left( d^2 - \frac{1}{3} db + b^2 \right) \\ & + B \left( e^2 + \frac{17}{9} ea + a^2 \right) \\ & + C \left( 2de + \frac{17}{9} ad - \frac{1}{3} be + \frac{1}{3} ab \right) \\ & + D \left( \frac{1}{3} ad - be - ab \right) = 0. \end{aligned}$$

And putting  $A = 6$ ,  $B = 9$ ,  $C = \frac{9}{2}$ ,  $D = -\frac{15}{2}$ , this is

$$\begin{aligned} & 9 \left( a^2 + \frac{17}{9} ae + e^2 \right) \\ & + 6 \left( b^2 - \frac{1}{3} bd + d^2 \right) \\ & + \frac{9}{2} \left( \frac{1}{3} ab + 2de + \frac{17}{9} ad - \frac{1}{3} be \right) \\ & + \frac{15}{2} \left( ab - \frac{1}{3} ad + be \right) = 0, \end{aligned}$$

which is the before-mentioned quadric surface; hence the quadric surface and the cubic surface intersect in the two lines

$$(d - \theta_1 b = 0, e - \theta_1^2 a = 0), (d - \theta_2 b = 0, e - \theta_2^2 a = 0)$$

(where  $\theta_1, \theta_2$  are the roots of the quadric equation  $\theta^2 - \frac{1}{3}\theta + 1 = 0$ );

and they consequently intersect also in an excubo-quartic curve, which is the theorem required to be proved.

Blackheath, March 26, 1864.

### LXVIII. On Kirchhoff's *Theory of Solar Spots*.

By P. A. SECCHI\*.

THE theory of solar spots proposed by Prof. Kirchhoff, in place of the one hitherto received, was briefly noticed by us in the *Bull. Meteorologico dell' Osserv. Romano* (No. 2, vol. ii.). The proposer having by his beautiful spectrum discoveries, meritoriously acquired an immense popularity and esteem, his theory has attracted considerable attention; a few remarks on the subject, therefore, based on the various researches which have recently been made, will not be out of place.

Prof. Kirchhoff rejects the theory of W. Herschel as well as that of Wilson; which two theories, we may here observe, are so far distinct that the confutation of the one does not necessarily involve the fall of the other. Herschel imagined the solar nucleus to be solid and dark-coloured, but surrounded by two strata of luminous clouds, separated from one another by a non-luminous stratum, and he attributed the spots to the rents in these envelopes. According to him, the nuclei consist of the comparatively dark body of the sun itself seen through apertures in both atmospheres; and the penumbrae are occasioned by the larger rents of the second luminous stratum. Prof. Kirchhoff objects to these two atmospheres—which, we admit, always appeared objectionable to us also, on the ground of their being unnecessary, and of their necessitating a simultaneous rent in both. Numerous careful observations made with powerful instruments led us some time ago to the conclusion that the penumbrae were, for the most part, formed either by filaceous currents of the matter which constitutes the sole photosphere enveloping the sun, or by the attenuation of the latter until it became transparent; at the same time we drew attention to the presence, within the spots, of *veli* and *cirri*, more luminous than their nuclei, but less so than their penumbrae—thus confirming the discovery of Dawes, who justly complains that hitherto attention has not been paid thereto by those who have occupied themselves with the subject.

\* From *Il Nuovo Cimento*, tomo xviii. p. 81.



The attention of observers has recently been drawn to the willow-leaf structure of the sun's envelope, indicated by Nasmyth. To this gentleman—who used a large reflector provided with oculars, not darkened, but in which the intensity of the light was diminished and rendered tolerable to the eye by reflexion from a glass plate—the sun appeared to be formed of innumerable bright elliptical patches, in the form of leaves, and superposed on each other; at the contours of the penumbrae they appeared to be more isolated, and resembled interlaced leaves, which projected separately into the interior of the nucleus itself.

Circumstances have not been sufficiently favourable as yet to permit us to recognize this structure; in this respect, indeed, Mr. Dawes has not been more fortunate, for he found the solar structure, described by Sir J. Herschel as consisting of a kind of luminous flakes, more in accordance with the phenomena observed by himself during many years of research; and with respect to the penumbrae, he admits that there are bright portions like currents, which, commencing in the nuclei, traverse the penumbrae, and retain all the brilliancy of the photosphere rather than of the penumbrae. This agrees with our own observations; for we have always insisted upon the three distinct constituents observable in every spot—*i. e.* the real nucleus, the penumbrae, and the semiluminous *cirri*. To explain this phenomenon it is not necessary to have recourse to two strata of luminous clouds; the assumption of a single incandescent photosphere mixed with less luminous vapours (as seen in eclipses) will suffice; through the rents made in this photosphere, by causes difficult to predict though easy to conjecture, the less bright central portion of the sun is seen.

This assumption of an obscure central part, however, is precisely what appears to be most absurd to Prof. Kirchhoff. “How is it possible,” says he, “to admit the absurd hypothesis according to which, at its contact with a body so incandescent as is the photosphere, and under so intense a radiation, the sun's nucleus has not yet arrived at a state of incandescence and fusion?” With all due respect to so distinguished a philosopher, we believe that there is here some exaggeration. In the first place, no one ever said that the nucleus is cold; and if obscure, its obscurity is merely relative. Galileo in his day asserted this; and photography proves that the chemical intensity of the nucleus is so great that, in order to obtain an image of the same, it is necessary to operate instantaneously, otherwise this image equals in brilliancy that of the photosphere. The difference in luminosity, therefore, is small; and if we could isolate one of these nuclei, we should probably find it difficult to distinguish the same from a portion of the sun itself. In order to prove

that the interior of the solar globe must be as bright as is the photosphere, Kirchhoff insists greatly on the principle that all substances become luminous at the same temperature. It appears to us that two very different things are here confounded—the invariability, for all bodies, of the temperature at which luminous rays, or such as affect the eye, begin to be emitted, and the *equal luminosity* of these bodies at the same temperature. We may admit the truth of the first of these propositions, and at the same time utterly deny that of the second. In furnaces we have gases and solids of very different luminosities; and is not the strongest known flame, that of the oxyhydrogen lamp, one of the least luminous? The conclusion, therefore, is altogether untenable that the parts which form the solar nucleus must be as luminous as the photosphere. From this, of course, it does not follow, either that the so-called nucleus must be solid, or that its temperature must be much lower, but merely that it is less luminous. It may be either liquid or gaseous; but in it that specific vivid action which characterizes the photosphere does not exist. As Loret well observes, the analogy with other planets indicates to us that the denser parts must be accumulated in the lower strata, and the lighter at the surface; and amongst the latter are the gases and more subtle materials from whose modifications the solar light results. Thus there is no absurdity whatever involved in the assumption that, underneath the highly incandescent stratum of the photosphere, there may be another, equally hot, possibly, but less luminous, which is rendered visible whenever the more incandescent stratum of the photosphere is itself rent asunder.

But careful consideration will show that it is impossible to admit an absolute identity of temperature in the various parts of the sun. In fact the incessant work which is performed in it, and the continual emission of heat, implies that one part must be actually *in* a state of chemical alteration, whilst another part must be on the point of entering that state. The first may be the photosphere, and the second the less luminous nucleus, just as is the case in an ordinary fire. Nor must we omit to observe that if the argument according to which all parts of the sun must have the same temperature be valid, it must apply with equal force to our furnaces also. We do not here compare the sun with a furnace in which wood burns; we assert merely that the work itself which is there performed, in order to preserve solar activity, involves the assumption of more and of less intense parts; otherwise we should be compelled to regard the sun as a merely incandescent body, whose light, as W. Thomson has shown, must necessarily be extinguished in some millions of years.

Again, to treat Wilson's theory as absurd shows that the same is confounded with that of Herschel, from which, in reality, it differs essentially. Wilson merely pronounced the spots to be cavities; and subsequent observations have proved this to be a *fact*. But no one said that these cavities were absolutely empty; on the contrary, the *cirri* seen across them show that the cavities are filled with less incandescent gaseous matter, which occasionally is visibly agitated, and thrown into vortices and currents. This being the case, what else can these cavities be, if not spaces full of less luminous and consequently less incandescent matter? Prof. Kirchhoff considers them to be clouds or masses at a lower temperature. When well considered, there is in reality not much difference between the two hypotheses, provided the terms are well defined. This difference is still further diminished when we find the origin of these clouds attributed to vortices and cataclysms, to which causes we, too, have frequently attributed the origin of spots.

The only controverted point which remains to be decided is whether the dark spot termed the nucleus is a portion of the general mass under the photosphere, or whether it is produced by the opacity of a cloud or more refrigerated mass, which intercepts from us the rays emitted by the subjacent luminous portion.

This question cannot be decided *à priori*; it will require an examination of the forms and phases of the spots themselves. Now their forms do not at all agree with those of clouds—so far, at least, as we can judge from the phenomena of our own atmosphere, and from the properties which an incandescent atmosphere, like that of the sun, may be reasonably supposed to possess.

In fact at the commencement the spots resemble black pores, which certainly suggest, to some extent, the idea of clouds. Every analogy, however, soon disappears; for as soon as the pore, by dilatation, assumes the appearance of a spot, its edges are seen to be serrated, and the penumbra is formed *entirely of very fine rays converging towards the centre of the figure*. As often remarked, the nucleus does not always present exactly the same contour as the penumbra, but to a *salient* angle of the luminous matter against the nucleus corresponds a *reentrant* one in the penumbra, exactly as if a descent of matter into the nucleus had taken place from its sides, and left an escarpment (talus) the more reentrant the more copious had been the detachment of matter. These are the phases through which all spots pass until they reach the first stage, which appears to be that of formation and complete development; to this succeeds the phase of dissolution.

Accordingly it is manifest that this first phase presents no-



thing analogous to that of the formation of a cloud. Each of these would make its appearance as a less luminous mass, and either be sharply separated all round from the other and hotter portions, like our *cumuli*, or be attenuated at the edges, like our *strati*. The above radiant form, and the appearance of currents precipitated into a cavity so as to form a perfectly distinct talus, could not at all occur, in accordance with what we see in the clouds of our own atmosphere, or with what we can conjecture. Any theory of the formation of spots must, in the first place, explain their shape; and this the cloud theory has hitherto failed to do.

When a spot has arrived at its full development, it presents to us a vast dark area; into the latter luminous threads are projected, like radiant torrents, from all the circumjacent photosphere, which penetrate, by long tortuous lines, into the interior of the nucleus, thus producing the long familiar appearance of rents. Now in our own atmosphere we never observe any such penetrations of heated into cooler matter, accompanied by constant and sharp separation of the two: nor, judging from analogy, does it appear possible that such could occur in the sun; for the opaque mass of cloud would either hide from our view the torrents which strove to penetrate therein with linear motion, or it would diminish the light of these torrents by cooling them. Now, as we have frequently observed, and in accordance with what Dawes has also recently stated (Phil. Mag. Feb. 1864, p. 156), the filaments of the photosphere, which penetrate the nuclei, preserve all the brilliant splendour of the photosphere itself. Such a structure of the spots certainly does not agree with the notion of clouds.

When a spot arrives at its last phase, previous to dissolution, the penumbra is less regularly radiated; it appears to be formed of the photosphere itself more attenuated and rarefied, so that at this stage it has some resemblance to a cloud; but, of course, a true theory must account for all phases. There is one circumstance, moreover, of which the cloud theory gives no account whatever; this is the presence of *faculæ* surrounding the spots.

These *faculæ*, as I long ago pointed out, are simply the crests of tempestuous waves, excited in the photosphere, which emerge from the denser stratum of the solar atmosphere; and they have every appearance of being formed of the matter of the photosphere whirled round by the internal force which creates the spot. If the spot were due merely to the formation of a cloud, one does not see why its contour should be agitated and violently ruptured. Everything indicates that the spots are centres of lower temperature; in fact I have shown this by the



thermoscope. At the same time it is manifest that the source of these gaps is rather an eruption proceeding from the interior of the nucleus, than a simple refrigeration produced in the photosphere by causes analogous to our own meteorological ones. In fact it is difficult to imagine the existence of the latter in the sun, whilst internal eruptions cannot fail to exist under the conditions to which that body is exposed.

Again, just as Herschel, in order to explain the penumbrae, was induced to assume two strata in the photosphere, so Prof. Kirchhoff requires two strata of clouds, necessarily coexistent and superposed one on the other. This hypothesis of two strata is in truth a mere expedient for the explanation of the penumbrae, whose constitution we have already described, and to explain which it is not necessary to introduce more than a simple photosphere, together with the accessories inseparable therefrom. The cloud hypothesis has often been reproduced, but always by those who have either observed the spots with imperfect instruments, or who have not studied them very carefully. By those who have had at their disposal better methods of observation, the hypothesis has always been rejected.

Neither is it at all the object, as we too frequently hear, of the supporters of the hypothesis of a less luminous nucleus to revive the old notion of the habitability of the sun; for if the Creator wished to make that planet habitable, He would not necessarily have to people it with men of flesh and bone like ourselves, who would be volatilized in a few instants; nor for this purpose is it necessary to suppose that the dark stratum acts as a tent to protect those imaginary inhabitants from the superior radiation. Such arguments have their appropriate place in treatises such as those of Fontanelle and his imitators, where they may serve to divert readers.

We merely assert that, without contradicting physical laws, *first*, the stratum of the photosphere may have a greater luminosity than the internal nucleus has; *secondly*, it is not necessary to suppose the so-called nucleus to be either solid or liquid; it may be gaseous and of greater density; *thirdly*, notwithstanding the proximity of the photospheric stratum, the nucleus may have not only a different degree of luminosity, but also a different temperature; *fourthly*, the apparent form of the spots absolutely excludes the hypothesis of a cloud structure, and we see nothing in them adequately analogous to the formation of terrestrial clouds, or to the phases through which they pass.

Our object in the preceding remarks has been, not so much to raise objections to the views of a distinguished physicist, as to prevent a scientific retrogression; for history shows that persons of great authority in one branch of science—but of less eminence

in others, which they have less profoundly studied—often misguide less experienced investigators by the weight of their opinion. We do not at all pretend to have given a true theory of the solar spots; we profess merely to have shown that the hypothesis which regards them as clouds is, of the many conceivable ones, decidedly not the happiest.

LXIX. *On the Electrical Relations of Metals &c. in Fused Substances.* By G. GORE, Esq.\*

**I**N the following experiments, most of the substances to be fused were contained in small porcelain crucibles, and heated by means of the flame of a Bunsen's burner; the more infusible bodies were melted in clay crucibles in one of my small gas-furnaces—particular care being taken to have neither an excess of gas nor of air in the furnace, by testing at the top of the chimney for carbonic oxide by means of a brightly red-hot rod of iron, and diminishing the supply of gas until all traces of carbonic oxide precisely disappeared. The condition of the contents of the crucible could at all times be ascertained without admitting air into the furnace, by placing upon the top of the furnace a vertical tube of fireclay about four inches high, closed at its upper end by a thin disk of glass.

The materials of which the electrodes were composed included carbon, magnesium, aluminium, silicium, zinc, tin, lead, iron, nickel, copper, silver, gold, and platinum. The carbon consisted of bars of gas-graphite as used for electric lamps; the magnesium was a rod of Mr. Sonstadt's purest variety; the aluminium was obtained from Messrs. Bell of Newcastle; the silicium was in the form of lumps, obtained by fusing the finest crystals under a mixture of powdered Bohemian glass, silico-fluoride of potassium, and a little hydrate of potash; the zinc, tin, and lead were of the best commercial kinds; the iron was "pianoforte wire;" the rod of nickel was kindly given to me by Mr. H. Wiggin, of the firm of Evans and Askin, Birmingham; the copper was ordinary wire of commerce; the silver and gold were of "virgin" quality; and the platinum was obtained from Messrs. Johnson and Matthey, London.

The most superficial consideration of the conditions of these experiments will show that a number of interfering circumstances were more or less necessarily present, and that the results obtained are not in all cases simply and purely due to chemico-electric action:—for instance, 1st, thermo-electric action of the heated and immersed ends of the electrodes; 2nd, ditto

\* Communicated by the Author.

of the ends of the electrodes out of the fused substance, which in many cases could not be removed a sufficient distance to be out of the influence of the heat; 3rd, the chemical influence of infusible or insoluble films formed upon the immersed electrodes; 4th, the accumulation of liquid of different composition around the electrodes; 5th, evolution of gases at the electrodes; 6th, alteration of structure of the electrodes by heat, their semi-fusion, &c.; 7th, currents occurring when by the relative coldness of the electrodes the fused salt around them solidified; and 8th, in addition to all these, the interference of impurities in the fused substances and in the electrodes themselves.

It will at once be seen that some of these interferences could not be prevented or avoided; with regard to the others, every reasonable precaution was taken, and the results in each uncertain case were properly verified.

In the following lists the most positive substance is in each case named first, and substances united by the mark — were about equally positive.

With *fused Boracic Acid* the following order of electrical relations was found. Iron, silicium—carbon, platinum, gold, copper, silver. The currents obtained were very feeble.

*Glacial Phosphoric Acid*.—Zinc, iron, copper, silver, platinum. The copper, iron, and zinc dissolved quickly; some of the gas evolved by the zinc exploded.

Large crystals of *Iodine* which had long been exposed in a bottle near lumps of fused chloride of calcium gave, when fused, feeble currents with silver and platinum, the former being positive.

*Fused Selenium* yielded no currents with platinum and copper or silver.

*Sulphate of Ammonia*.—Zinc, copper, silver, iron, platinum, carbon. Copper evolved much gas and dissolved violently.

*Nitrate of Ammonia*.—Magnesium, zinc, lead, copper, silver, tin, aluminium, iron, silicium—carbon, platinum. Lead was very strongly positive to copper without manifesting strong chemical action, zinc evolved gas violently.

*Hydrate of Potash*.—Silicium, aluminium, zinc, iron, lead (?), carbon, copper, platinum, silver. Silicium was strongly positive to aluminium, and strongly acted upon, with evolution of gas. The results were variable with copper and platinum, and with platinum and silver.

*Borate of Potash*.—Iron, zinc, copper, silver, platinum.

*Phosphate of Potash*.—Zinc, iron, copper, silver, platinum.

*Sulphide of Potassium*.—Aluminium, zinc, copper, silver, platinum, carbon, iron. Copper and silver dissolved rapidly with violent action. Reversals of current occurred with platinum and



aluminium, aluminium and zinc, zinc and platinum, iron and carbon, silver and copper.

*Iodide of Potassium*.—Aluminium, zinc, iron, silver, copper, platinum. Reversals of current with copper and silver, copper and platinum, iron and zinc, probably from salt solidifying around the electrodes and subsequently fusing.

*Bromide of Potassium*.—Zinc, iron, copper, silver, platinum. Reversal of current with iron and zinc.

*Chloride of Potassium*.—Aluminium, zinc, iron, copper, silver, platinum. Reversals with silver and platinum, silver and copper, silver and iron, iron and copper, iron and zinc.

*Chlorate of Potash*.—Zinc, aluminium—iron, silver, copper, platinum. The currents were feeble, they were also indefinite until gas was evolved.

*Nitrate of Potash*.—Aluminium, zinc, copper, silver, iron, platinum.

*Nitrate of Potash*.—Tin, lead, aluminium, carbon, copper, iron, zinc, platinum, silver. Currents very feeble. Reversals with iron and platinum, iron and silver, carbon and copper. Carbon evolved much gas.

*Hydrate of Soda*.—Zinc, iron, carbon, copper, silver, platinum. Reversals with silver and platinum, copper and iron, carbon and iron. Moving either platinum or silver made each more positive.

*Biborate of Soda*.—Zinc, carbon, iron, copper, silver, platinum. Iron and zinc evolved gas.

*Mono-pyrophosphate of Soda*.—Iron, copper, silver, carbon, platinum. Reversal with silver and carbon.

*Sulphide of Sodium*.—Zinc, copper, silver, iron, platinum, carbon. Reversals with iron and carbon, iron and platinum, iron and silver. Iron much dissolved.

*Hyposulphite of Soda*.—Aqueous fusion. Zinc, carbon, copper, silver, iron, platinum.

*Bisulphate of Soda*.—Aqueous fusion. Iron, copper, silver, carbon, platinum. Reversal with iron and platinum. Gas evolved by iron.

*Iodide of Sodium*.—Iron, silver, copper, carbon, platinum. Reversals with platinum and silver, platinum and copper, platinum and carbon, carbon and copper, copper and silver, copper and iron, silver and iron.

*Bromide of Sodium*.—Zinc, iron, copper, silver, platinum. Reversals with platinum and silver, iron and zinc.

*Chloride of Sodium*.—Iron, copper, carbon, silver, gold, platinum. Reversals with carbon and copper, carbon and iron.

*Nitrate of Soda*.—Aluminium, zinc, carbon, copper, silver, iron, platinum. Reversals with iron and silver, iron and copper, carbon and copper. Carbon evolved much gas.



*Microcosmic Salt*.—Aqueous fusion. Zinc, iron, silver, copper — platinum, carbon.

*A mixture of Carbonates of Potash and Soda*.—Silicium, iron, zinc, carbon, copper, silver, platinum. Reversals with silver and platinum, silver and copper, copper and carbon. Silicium was strongly positive to iron, and much action and gas at the surface of silicium.

*A mixture of Fluorides of Potassium and Sodium*.—Silicium, iron, carbon, copper, silver, platinum. Electric currents strong. Silicium rapidly acted upon and dissolved, evolving much gas.

*Carbonate of Lithia*.—Iron, carbon, copper, silver, platinum. Reversals with platinum and silver, copper and carbon. Much effervescence.

*Iodide of Barium*.—Iron, carbon, silver — copper, platinum. Reversal with copper and silver; either was positive when moved. Silver freely dissolved.

*Bromide of Barium*.—Iron, carbon, copper, silver, platinum. Reversals with silver and copper, iron and copper, iron and carbon.

*Nitrate of Baryta*.—Zinc, carbon — copper — silver, iron, platinum. Reversal with copper and silver. Carbon evolved much gas, which appeared to make it negative to copper, and to silver especially. Iron evolved much gas.

*A mixture of Caustic Baryta and the Fluorides of Potassium and Sodium*. Silicium, iron, carbon, copper, silver, platinum. Carbon evolved gas, iron much gas, and silicium very much gas.

*Nitrate of Strontia*.—Carbon, platinum, iron. Reversal with iron and platinum.

*Iodide of Calcium*.—Aqueous fusion. Silver positive to platinum.

*Bromide of Calcium*.—Aqueous fusion. Zinc, copper, iron, platinum.

*Nitrate of Lime*.—Silver, copper, platinum, iron, carbon.

*Nitrate of Magnesia*.—Aqueous fusion. Zinc, silver, iron, silicium, carbon, platinum.

*Silicate of Potash* (soluble glass).—Copper, silver, platinum.

*Bohemian Glass*.—Carbon positive to iron, current feeble.

*A mixture of fine white Sand and Hydrate of Potash*. Nickel positive to carbon.

*Silico-fluoride of Potassium*.—Silicium, iron, carbon, copper, silver, platinum.—Iron and silicium evolved gas.

*A mixture of Soda-lime, white Sand, and Hydrate of Soda*.—Carbon strongly positive to nickel.

*Tungstate of Soda*.—Aluminium, iron, copper, silicium, carbon, silver, gold, platinum. Reversal with gold and carbon.

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*Molybdic Acid*.—Copper, silver, platinum, carbon. Silver and copper acted upon.

*Bichromate of Potash*.—Silver, copper, silicium, carbon, iron, platinum, gold. Reversal with copper and silver.

*Chromate of Soda*.—Iron, copper, carbon, silver, platinum, gold. Currents strong.

*Chloride of Manganese*.—Copper, silver, iron, carbon, gold, platinum. Currents strong.

*Bisulphide of Arsenic*.—Silver, iron, carbon. Silver dissolved rapidly.

*Tersulphide of Arsenic*.—Copper, iron, platinum.

*Teriodide of Arsenic*.—Silver positive to platinum. Bad conductor.

*Arsenite of Soda*.—Iron, silicium, carbon — silver, copper, platinum. Reversals with silver and carbon. Iron, silicium, and carbon evolved much gas. Platinum, copper, and silver rapidly melted in it.

*Teroxide of Antimony*.—Silicium, iron, carbon, copper — silver, platinum. Reversal with silver and copper.

*Tersulphide of Antimony*.—Silver, copper, zinc, iron, silicium, platinum, carbon. Reversals with platinum and iron, copper and silver. Copper and silver dissolved quickly.

*Teriodide of Antimony*.—Silver, zinc, copper, iron, carbon, platinum. Currents feeble.

*Terbromide of Antimony*.—Copper, silver, zinc, iron, platinum, carbon. Reversal with zinc and iron. Currents very feeble.

*Oxybromide of Antimony*.—Silicium, iron, copper, silver, carbon, platinum, gold. Silver and copper dissolved quickly.

*Fluoride of Antimony*.—Zinc, iron, copper, silver, carbon, platinum. Iron coated itself black.

*Bromide of Zinc*.—Copper, iron, silver, platinum.

*Chloride of Cadmium*.—Aluminium, zinc, iron, copper, silver, platinum. Reversal with silver and copper. Zinc melted quickly; aluminium similar, with violent action.

*Mineral Phosphate of Lead*.—Iron positive to carbon. Iron dissolved with strong action; gas also evolved from carbon. Much lead reduced to a metallic button.

*Iodide of Lead*.—Zinc, silver, copper, iron, platinum. Reversals with platinum and iron, silver and zinc. Copper and silver dissolved rapidly.

*Chloride of Lead*.—Zinc, lead, copper, iron, silver, platinum. Reversals with copper and silver, copper and iron.

*Chromate of Lead*.—Carbon positive to iron. Strong action and intense heat at surface of iron. Iron much dissolved; carbon also corroded.

*A mixture of black scale Oxide of Iron and Silica.*—Iron, carbon, platinum. Reversal with iron and carbon. Current strong with iron and platinum. Iron dissolved.

*Protoxide of Copper.*—Platinum positive to iron as long as iron was enveloped by an unfused coating of oxide of copper, then violent action upon the iron attended by evolution of intense heat and very rapid solution of the iron, the iron at the same time becoming strongly positive to platinum. A large button of reduced copper was formed. Protoxide of copper appears to evolve oxygen gas when fused.

The following inferences may be deduced from these experiments. The most negative substances in fused salts are generally platinum, gold, carbon, and silver; the most positive substances are generally magnesium, aluminium, and zinc. Silicium is generally electro-positive to carbon; and is strongly positive and quickly corroded in fused alkalies, alkaline carbonates, or fluorides. Carbon is not generally very positive to iron.

The following facts may also be noticed. Copper and silver dissolve rapidly in fused sulphide of potassium, tersulphide of antimony, or iodide of lead; silver also dissolves freely in iodide of barium, or bisulphide of arsenic. Platinum, copper, and silver rapidly melt in fused arsenite of soda. Electric currents were repeatedly observed whilst one (or both) of the electrodes was coated with unfused salt, and on the fusion of the saline coating strong currents in an opposite direction generally occurred.

The investigation was very suggestive of new experiments; it suggested the examination of the various phenomena which acted as sources of interference, which I have already enumerated; it also threw light upon the desirable object of obtaining a cheap source of electricity by the combustion of coke or gas-carbon. The discovery of some suitable fused salt or mixture in which carbon is highly electro-positive at a high temperature to iron, nickel, or other infusible and suitable conductor, would probably prove a cheap and powerful source of electricity; cheap because of the low equivalent number of carbon and the low price of coke and gas-graphite, and powerful because of the intense affinity of carbon for oxygen at high temperatures, sufficient indeed to set the alkali-metals free from their oxides. The nearest approach to this object in these experiments was with carbon and nickel in a fused mixture of soda, lime, and silica.

LXX. *Researches in Hydrodynamics, with reference to a Theory of the Dispersion of Light.* By Professor CHALLIS, M.A., F.R.S., F.R.A.S.\*

THE theory of Double Refraction on the hypothesis of undulations, given in the Number of the Philosophical Magazine for last December, involved the theory of Dispersion on the same hypothesis; but for the purpose I had there in view it was not necessary to discuss fully the results obtained relative to the latter theory. On subsequently testing by numerical examples the equation ( $\alpha$ ), which gives the law of dependence of the refractive index ( $\mu$ ) on the breadth of the wave ( $\lambda$ ), I met with an incompatibility which proved that the true formula for dispersion had not been discovered. Either, therefore, the hypotheses of the theory were false and must be given up, or, if true, I had reasoned from them erroneously. In a difficult investigation of this kind, involving necessarily definite hypotheses respecting the elementary composition of the refractive medium, failure might be attributable to error in these hypotheses, while at the same time the antecedent hypotheses respecting the properties of the æther, and the laws of its motion and pressure deduced therefrom mathematically, might still be true. In order to be able to decide whether or not the failure was due to erroneous reasoning from the supposed constitution of the refractive medium, I reconsidered the mathematical argument, and found, in fact, that a step in it had been omitted, which it will be the object of this and a succeeding communication to point out and to supply. I discovered also that the expressions for  $\frac{d\lambda}{d\mu}$  and  $\frac{d.e^2}{d\mu}$  had been incorrectly deduced from the equation ( $\alpha$ ); but as the correct expressions would have led to the same results, and as that equation itself requires modification, it is needless to rectify the error.

According to the theory, the diminution of the rate of propagation of an æthereal wave after entrance into a medium, is produced by the mean retarding effect of reflexions of the motion from the atoms, an extremely large number of atoms being supposed to be contained in a space the linear dimensions of which are extremely small compared with  $\lambda$ . The amount of the retardation depends in part on the mobility of the atoms, which may be conceived to be in positions of equilibrium by reason of the action of attractive and repulsive molecular forces, and consequently to be moveable by any extraneous action, such as that of the ætherial waves. Both the theory of double refraction and that of dispersion require a mathematical investigation of the modifying effect of the motion of the atoms; and for this pur-

\* Communicated by the Author.



pose it is necessary to take account of the proper elasticity of the refractive medium in investigating an expression for the acceleration of an individual atom due to the dynamical action of the æthereal waves. In seeking for such an expression, I supposed at first that the atoms were fixed, and assumed that the condensation or rarefaction of the æther in contact with any atom is always proportional to the condensation or rarefaction of the wave at the position where the atom is situated, but is distributed unequally about the surface, the excess being on the hemisphere on which the waves are incident, and the distribution being symmetrical about an axis drawn through the centre in the direction of incidence. As the resulting accelerative force in that direction would thus be proportional to the condensation, and consequently to the velocity of the incident waves, the expression assumed for it was

$$km \sin \frac{2\pi}{\lambda} (\kappa at - x + c),$$

$k$  being a constant factor multiplying the velocity. At this point of the reasoning I omitted to inquire whether that factor is a function of  $\lambda$ , and subsequently treated it as if it were independent of that quantity. This inquiry I now propose to enter upon.

The whole investigation, it will eventually appear, turns upon the solution of the following hydrodynamical problem:—A series of waves defined by the equations

$$V = \kappa a \sigma = m \sin \frac{2\pi}{\lambda} (\kappa at - x + c),$$

is incident in a given direction on a fixed smooth sphere of given radius: it is required to find the condensation at any point of the surface of the sphere at any instant. Since in the view I take of theoretical physics this problem is of the utmost importance, I have given to it especial consideration; and though I seem to have succeeded to a considerable extent in solving it, I do not profess to have completely overcome the difficulties. The process I have adopted depends in part on the new general hydrodynamical equation, which I have deduced from the principle of the continuity of successive surfaces of displacement of a given element in successive instants. I am aware that the solution might be attempted without reference to that equation by a process analogous to that employed by Poisson in determining the simultaneous movements of a ball pendulum and the surrounding air. But with every disposition to find that that method is exact and sufficient, I have been compelled, for reasons which will appear in the sequel, to have recourse to inferences drawn from the new equation. My previous researches having demonstrated that the

determination of the motion and propagation of plane waves, when deduced from only two fundamental equations, presents contradictory results which disappear when the third equation is also employed, it is not allowable to dispense with the use of this equation in any case. That one instance suffices to prove that inferences from the two equations alone, even if not self-contradictory, are not necessarily true. For the purpose of exhibiting clearly the distinction between the two processes, and giving the opportunity of judging of their respective merits, I shall attempt the solution of the proposed problem, first, by employing only the two usual fundamental equations, and then by joining with them the third.

The reasoning being restricted to the first powers of the velocity and condensation, we have the usual equations

$$\frac{a^2 d\sigma}{dx} + \frac{du}{dt} = 0, \quad \frac{a^2 d\sigma}{dy} + \frac{dv}{dt} = 0, \quad \frac{a^2 d\sigma}{dz} + \frac{dw}{dt} = 0,$$

and

$$\frac{d\sigma}{dt} + \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0.$$

Differentiating the last equation with respect to  $t$ , and substituting from the other three, we obtain

$$\frac{d^2\sigma}{dt^2} = a^2 \left( \frac{d^2\sigma}{dx^2} + \frac{d^2\sigma}{dy^2} + \frac{d^2\sigma}{dz^2} \right).$$

Now, from the conditions of the problem, the condensation at any given point must be a periodic function of the time, the period being the same as that of the incident waves. Hence we may assume generally that  $\sigma = f_1(t)\phi_1(x, y, z) + f_2(t)\phi_2(x, y, z) + \&c.$ , provided the functions  $f_1, f_2, \&c.$  satisfy the equations

$$f_1''(t) + n^2 f_1(t) = 0, \quad f_2''(t) + n^2 f_2(t) = 0, \quad \&c.,$$

$n$  being put for  $\frac{2\pi a}{\lambda}$ . Since from this value of  $\sigma$ ,  $\frac{d^2\sigma}{dt^2} + n^2\sigma = 0$ , it follows by substitution in the foregoing equation that

$$\frac{d^2\sigma}{dx^2} + \frac{d^2\sigma}{dy^2} + \frac{d^2\sigma}{dz^2} + \frac{n^2\sigma}{a^2} = 0.$$

As by hypothesis the condensation is symmetrically disposed about an axis drawn through the centre of the sphere in the direction of the incidence of the waves, its value at any point at a given instant is a function of the straight line ( $r$ ) joining the point and the centre of the sphere, and the angle ( $\theta$ ) which this line makes with the axis. Hence the last equation may be transformed into one containing only the polar coordinates  $r$  and  $\theta$ . The trans-

formation gives

$$\frac{d^2 \cdot r \sigma}{dr^2} + \frac{1}{r^2} \left( \frac{d^2 \cdot r \sigma}{d\theta^2} + \frac{d \cdot r \sigma}{d\theta} \cot \theta \right) + \frac{n^2 r \sigma}{a^2} = 0.$$

Having regard to the ultimate application of this analysis, I shall now assume the magnitude of the sphere to be so extremely small that the distance from its centre to which any sensible effect on the incident waves extends is very small compared to  $\lambda$ . In that case, since  $\frac{n^2}{a^2} = \frac{4\pi^2}{\lambda^2}$ , the last term of the above equation may be omitted. Then, to obtain a particular solution of this equation, let it be assumed that

$$\frac{d \cdot r \sigma}{d\theta} = f_1 \psi_1 \sin \theta,$$

$f_1$  being, in accordance with what is said above, a periodic function of the time, and  $\psi_1$  being supposed to be a function of  $r$  only. Hence by substitution, after differentiating the equation with respect to  $\theta$ , the result will be

$$f_1 \left( \frac{d^2 \psi_1}{dr^2} - \frac{2\psi_1}{r^2} \right) \sin \theta = 0,$$

which is consistent with the supposition that  $\psi_1$  is a function of  $r$  and constants only. By integration,

$$\psi_1 = \frac{c_1}{r} + c_2 r^2.$$

Now if  $U$  be the velocity in the direction of  $r$  at any point whose coordinates are  $r$  and  $\theta$ , and if  $W$  be the velocity perpendicular to  $r$ , and be supposed positive in the direction in which  $\theta$  increases, we shall have

$$\frac{a^2 d\sigma}{dr} + \frac{dU}{dt} = 0, \text{ and } \frac{a^2 d\sigma}{r d\theta} + \frac{dW}{dt} = 0.$$

But from the expression for  $\frac{d \cdot r \sigma}{d\theta}$  above, it follows, after substituting the value of  $\psi_1$ , that

$$\frac{d\sigma}{d\theta} = f_1 \left( \frac{c_1}{r^2} + c_2 r \right) \sin \theta.$$

Hence by integration,

$$\sigma = \phi(r, t) - f_1 \left( \frac{c_1}{r^2} + c_2 r \right) \cos \theta.$$

It is here to be remarked that this value of  $\sigma$  contains a term increasing indefinitely with  $r$  only because the analytical reasoning has been conducted approximately, as will appear from the

following considerations. Let the condensation be  $\sigma_1$  at a point for which the value of  $r$  is so large as to make the term  $\frac{c_1}{r^2}$  insensible, and where, consequently, the effect of the disturbance of the incident waves vanishes. Then

$$\sigma_1 = \phi(r, t) - f_1 c_2 r \cos \theta.$$

But by the given conditions of the problem,

$$V = a\sigma_1 = m \sin \frac{2\pi}{\lambda} (at - x + c),$$

the factor  $\kappa$  being unity in the process of investigation now employed, and the coordinate  $x$  being reckoned positive in the direction of propagation. Hence, taking the origin of  $x$  to be the centre of the sphere, we shall have  $-x = r \cos \theta$ ; and by what is said above, this coordinate may always be regarded as very small compared to  $\lambda$ . Thus, very nearly,

$$\sigma_1 = \frac{m}{a} \sin \frac{2\pi}{\lambda} (at + c) + \frac{2\pi m}{\lambda a} r \cos \theta \cos \frac{2\pi}{\lambda} (at + c).$$

Comparing this value of  $\sigma_1$  with that above, it follows that

$$\phi(r, t) = \frac{m}{a} \sin \frac{2\pi}{\lambda} (at + c), \text{ and } -f_1 c_2 = \frac{2\pi m}{\lambda a} \cos \frac{2\pi}{\lambda} (at + c).$$

Hence also

$$\frac{d\sigma}{dr} = f_1 \left( \frac{2c_1}{r^3} - c_2 \right) \cos \theta.$$

By substituting the values of  $\frac{d\sigma}{d\theta}$  and  $\frac{d\sigma}{dr}$  that have been thus obtained, we have

$$\frac{dU}{dt} = -f_1 a^2 \left( \frac{2c_1}{r^3} - c_2 \right) \cos \theta, \quad \frac{dW}{dt} = -f_1 a^2 \left( \frac{c_1}{r^3} + c_2 \right) \sin \theta.$$

The integration of these equations gives, regard being had to the preceding value of  $f_1 c_2$ ,

$$U = \left( \frac{2c_1}{c_2 r^3} - 1 \right) m \cos \theta \sin \frac{2\pi}{\lambda} (at + c),$$

$$W = \left( \frac{c_1}{c_2 r^3} + 1 \right) m \sin \theta \sin \frac{2\pi}{\lambda} (at + c),$$

no arbitrary function of coordinates being added, because  $U$  and  $W$  are by hypothesis periodic functions of the time. The only remaining condition to be satisfied is that  $U=0$  at every point of the surface of the sphere. If, therefore,  $b$  be the radius of



the sphere, we shall have  $\frac{c_1}{c_2} = \frac{b^3}{2}$ ; and thus, by substitution in the above equations, the values of  $U$  and  $W$  are completely determined. Also by substituting the values of  $-f_1 c_2$  and  $\frac{c_1}{c_2}$  in the expression for  $\sigma$ , the condensation is given to the same approximation by the equation

$$\sigma = \frac{m}{a} \cdot \left\{ \sin \frac{2\pi}{\lambda} (at + c) + \frac{\pi}{\lambda} \left( \frac{b^3}{r^2} + 2r \right) \cos \theta \cos \frac{2\pi}{\lambda} (at + c) \right\}.$$

We have thus solved the proposed problem on the principles that are usually adopted.

In order to verify the foregoing reasoning, I shall now show that the problem of the simultaneous movements of a vibrating sphere and the surrounding air may be solved by a like process, and that the solution thus obtained is identical with that of Poisson. The centre of the sphere being assumed to be always on a fixed straight line, let  $\alpha$  be its distance from a fixed point of that line at any time  $t$ , and let the moving centre be the origin of the coordinates  $x, y, z$  of any point of the fluid, the axis of  $x$  coinciding with the fixed line. Then

$$\sigma = f_1(t) \phi_1(x - \alpha, y, z) + f_2(t) \phi_2(x - \alpha, y, z) + \&c.$$

On obtaining from this equation  $\frac{d\sigma}{dt}$  and  $\frac{d^2\sigma}{dt^2}$ , terms will arise

which will have  $f_1(t)$  and  $\frac{d\alpha}{dt}$ ,  $f_2(t)$  and  $\frac{d\alpha}{dt}$ , &c. as factors, and which will consequently be of the order of the *square* of the velocity. These being omitted, the differential equation of which  $\sigma$  is the principal variable will be precisely the same as in the foregoing problem, and its integral will be the same; but the arbitrary quantities introduced by the integration will have to be differently determined. Reverting to the expression for  $\sigma$ , since the condition is to be fulfilled that  $\sigma = 0$  for all except very small values of  $r$ , it follows that  $\phi(r, t) = 0$ , and  $c_2 = 0$ . Consequently

$$\sigma = -\frac{f_1 c_1}{r^2} \cos \theta, \quad \frac{d\sigma}{dr} = \frac{2f_1 c_1}{r^3} \cos \theta,$$

$$\frac{dU}{dt} = -\frac{2f_1 c_1 a^2}{r^3} \cos \theta, \quad \frac{dW}{dt} = -\frac{f_1 c_1 a^2}{r^3} \sin \theta.$$

Now supposing the velocity of the centre of the sphere to be  $T$ , the only condition that remains to be satisfied is that where  $r = b$ ,  $U = T \cos \theta$ , the fluid in contact with the sphere necessarily

moving in the normal direction with the same velocity as the points of the sphere with which it is in contact. Hence

$$-\frac{2c_1a^2}{b^3} \cos \theta \int f_1 dt = T \cos \theta.$$

By means of this equation it will readily be found that

$$U = \frac{Tb^3}{r^3} \cos \theta, \quad W = \frac{Tb^3}{2r^3} \sin \theta, \quad a^2\sigma = \frac{dT}{dt} \cdot \frac{b^3}{2r^2} \cos \theta.$$

As these results agree exactly with those of Poisson's solution, they may be considered to prove that the principles of the two solutions are the same, although the mathematical processes are considerably different. It may also be concluded that the case of waves impinging on a sphere at rest is treated above in exact accordance with the reasoning usually adopted in hydrodynamical questions. Waiving for the present the statement of the difficulties presented by these results, I shall now only direct attention to two inferences which may be drawn from them. First, the solution of the first problem might be deduced from that of the other, so far at least as regards the *velocity* of the fluid, by conceiving a velocity equal to that of the vibrating sphere to be impressed at each instant on the sphere and on the fluid in the contrary direction, so as to reduce the sphere to rest. Secondly, the velocity of the fluid in contact with the fixed sphere, where it passes the plane through the centre of the sphere perpendicular to the direction of the incidence of the waves, is to the velocity in the undisturbed wave as 3 to 2. Hence it will be found, by calculating according to the above law of the variation of  $W$  inversely as  $r^3$ , that the whole quantity of fluid which passes that plane is just as much as would have passed it if the waves had been undisturbed, and that thus the mean quantity which at each instant is diverted in the contrary direction is zero. So also the mean quantity of the fluid which the vibrating sphere pushes or draws at each instant in the direction of its motion is zero. I proceed now to apply to the same two problems the hydrodynamical principles which I long since enunciated in this Journal.

After expressing the law that the lines of motion are normals to a continuous surface by the equation

$$(d\psi) = \frac{u}{\lambda} dx + \frac{v}{\lambda} dy + \frac{W}{\lambda} dz,$$

an equation necessary for determining the unknown function  $\lambda$  was obtained on the principle that that law of continuity holds good for any given particle in successive instants, or, that the motion conforms to the law at all points and at all times. In like manner the equation is obtained which expresses that the

motion is always and everywhere consistent with the principle of constancy of mass. The equation deduced from the above principle of continuity is

$$\frac{d\psi}{dt} + \lambda \left( \frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right) = 0,$$

from which we may, in the first place, infer that, as the complete value of  $\psi$  will contain arbitrary functions of the coordinates and the time, there may be an unlimited number of ways in which  $udx + vdy + wdz$  can be made integrable by a factor, the particular factor in each case being determined by the arbitrary circumstances of the motion. This inference is true whether the motion be large or small, and whether it be vibratory or not. At the same time, as is indicated by pure analysis, if  $udx + vdy + wdz$  be integrable without a factor, the quantity  $\lambda$  must be supposed to be generally a function of  $\psi$  and  $t$ . This analytical circumstance must have its counterpart in the motion of the fluid. In fact I have proved that when that supposition relative to  $\lambda$  is introduced into the foregoing equation, the consequent value of the function  $\psi$  shows that the motion is *rectilinear*. As this result has been reached irrespectively of any arbitrary conditions that might produce rectilinear motion, we may conclude that the rectilinearity is due to the mutual action of the parts of the fluid, and that, so far as that action is unconstrained, there exist, under all circumstances of disturbance, straight lines, or *axes*, along which the motion takes place. But we have no right to conclude that the motion is wholly, or necessarily rectilinear.

Again, from the three approximate dynamical equations aduced at the commencement of this reasoning, I have demonstrated generally that if  $udx + vdy + wdz$  be integrable for small motions, the motion cannot be independent of the time, but must consist of small *vibratory* motions. Here also a characteristic of the motion has been indicated by the analysis antecedently to any supposed conditions of the motion; and on this account we are entitled to conclude that vibratory motion results generally from the mutual action of the parts of the fluid. But we have no right to say that in every *arbitrary* case of small vibratory motion  $udx + vdy + wdz$  is an exact differential. This assertion, which is assumed to be true in the ordinary treatment of hydrodynamical questions by two general equations, is contradicted by the third one above, which, as we have already argued, proves generally, and prior to any consideration of vibratory motion, that that differential equation becomes an exact differential by being multiplied by a factor, the value of which depends in each particular case on the arbitrary circumstances of the motion.

Since from the same three approximate differential equations,



combined with the fourth expressing the principle of constancy of mass, a linear differential equation, having constant coefficients and  $\sigma$  for principal variable, was obtained antecedently to any supposed case of motion, it follows that the mutual action of the parts of the fluid is such as to be consistent with the coexistence of small motions.

Having thus indicated the processes of reasoning which conducted to *rectilinear axes* of motion, to *vibratory motion*, and to the *coexistence of vibrations*, prior to supposing any case of motion, and having elsewhere given in full the investigation of exact analytical expressions for the vibrations parallel and transverse to the rectilinear axis (see the Philosophical Magazine for May 1849), I have now to add a remark of essential importance in the present inquiry. On passing from these antecedent general inferences to the consideration of particular cases of disturbance of the fluid, it is necessary to proceed on the principle that the state of the fluid as to velocity and condensation is both initially and subsequently consistent with the previously demonstrated general characteristics. Thus, for instance, a series of plane waves must be conceived to be composed of vibrations parallel and transverse to rectilinear axes unlimited in number and all perpendicular to the plane fronts of the waves. This instance I have especially considered in the article on Double Refraction in the December Number, where reasons are also given for concluding that in consequence of this composition of vibrations lateral divergence is prevented, and the motion might be comprised within a cylindrical space of very small transverse section. This result from the mathematical theory of undulations is singularly in accordance with the phenomenon of the transmission of a *thread* or *pencil* of light,—a very remarkable fact, of which I am not aware that any theoretical explanation had previously been given. The result is also worthy of notice as presenting an instance of small vibrations for which  $u dx + v dy + w dz$  is clearly not an exact differential. Further, it is to be observed that for this composite motion, supposing it to be wholly parallel to the axis of  $x$ , the dynamical equation becomes

$$\kappa^2 a^2 \cdot \frac{d\sigma}{dx} + \frac{du}{dt} = 0,$$

the factor  $\kappa^2$  being introduced, as I have shown in the Philosophical Magazine for November 1853, in passing from free to constrained rectilinear vibrations.

In the case for which the vibratory motion is central and is supposed to be a function of the distance from the centre, account must still be taken of the same law of the composition of vibrations. Since, as in the preceding case, the resulting motion



is wholly rectilinear, the number of the rectilinear axes must be conceived to be so great that the transverse motions are mutually destructive. We may, if we please, suppose the evanescence of the transverse motions to result from the motion being composed of an unlimited number of such motions in cylindrical threads as those which are shown above to be physically possible, the axes of the cylinders all passing through the centre. This being understood, let us consider by itself the motion within a pyramidal space having its vertex at the centre, and its vertical angle indefinitely small. If  $V$  be the velocity and  $\sigma$  the condensation at the distance  $r$  from the centre, we shall have, as in the case of plane waves, to the first order of small quantities,

$$\kappa^2 a^2 \cdot \frac{d\sigma}{dr} + \frac{dV}{dt} = 0.$$

Also if the lines of motion, that is, the lines drawn always in the direction of the motion of the particles through which they pass, all converge to a *moving* centre, the same equation still applies to the motion within each small pyramidal space, although the total motion in this case may not be alike in all directions from the centre. And generally, supposing the lines of motion and surfaces of displacement to be of any form whatever, and either to be fixed in space, or to vary in position with the time, provided there is no *abrupt* change of direction in passing at a given instant from a point on one line to an adjacent point of a contiguous line, or from point to point of a *given* line, and no extraneous force acts, we have

$$\kappa^2 a^2 \cdot \frac{d\sigma}{ds} + \frac{dV}{dt} = 0,$$

the line  $s$  being measured along a line of motion from a given origin on the same, and  $V$  being the total velocity. These theorems are stated here because they will be employed subsequently; but for their demonstration I must refer to my previous hydrodynamical researches, especially those contained in the Numbers of the Philosophical Magazine for February and November 1853.

Returning now to the case in which the lines of motion are rectilinear and pass through either a fixed or a moving centre, and joining with the foregoing equation that of constancy of mass to the same approximation, namely,

$$\frac{d\sigma}{dt} + \frac{dV}{dr} + \frac{2V}{r} = 0,$$

we shall obtain

$$\frac{d^2 \cdot r \sigma}{dt^2} = \kappa^2 a^2 \cdot \frac{d^2 \cdot r \sigma}{dr^2}, \quad \kappa a \sigma = \frac{f'(\kappa a t - r + c)}{r}$$

$$V = \frac{f'(\kappa a t - r + c)}{r} - \frac{f'(\kappa a t - r + c)}{r^2},$$

the propagation being exclusively from the centre. Since in the applications about to be made of these equations the function  $f$  will be of the form  $m \sin \frac{2\pi}{\lambda} (\kappa a t - r + c)$ , and the value of  $\lambda$  is supposed to be extremely large compared to the values of  $r$  concerned in the investigation, it is allowable to omit the terms containing  $f'$ , and to take  $V$  to be equal to  $\frac{\phi(t)}{r^2}$ . This

is, in fact, supposing, as was done in the other solution, the fluid to be incompressible within the small space over which the action of the small sphere is of sensible magnitude, so far at least as regards the effect of that action. It should, however, be noticed that the complete values of  $\sigma$  and  $V$  prove that the condensation and velocity impressed by the sphere are *propagated* with the velocity  $\kappa a$  to an unlimited distance, the fluid being, by supposition, unconfined. We are now prepared to enter upon the second solution of the problem of the vibrating sphere.

In the first place, as the sphere impresses motion only in directions perpendicular to its surface, the directions of the rectilinear axes of the component motions, being determined by that circumstance alone, must be the same. That is, as we have supposed the fluid to comport itself as if it were incompressible, the instantaneous lines of motion will be in the directions of the prolonged radii. But it is to be observed that under these circumstances the motion of a *given* particle is curvilinear. Let  $T$  be the velocity of the vibrating sphere. Then, according to the foregoing reasoning, the velocity  $V$  of the fluid at any point whose polar coordinates, referred to the moving centre of the sphere, are  $r$  and  $\theta$ , will be wholly in the direction of  $r$ , and be equal to  $\frac{Tb^2}{r^2} \cos \theta$ ,  $b$  being the radius of the sphere. Hence

$$\kappa^2 a^2 \cdot \frac{d\sigma}{dr} + \frac{b^2 \cos \theta}{r^2} \cdot \frac{dT}{dt} = 0;$$

and by integration, performed necessarily along the line of motion,

$$\kappa^2 a^2 \sigma = \frac{b^2 \cos \theta}{r} \cdot \frac{dT}{dt}.$$

No arbitrary function of  $t$  is added, because the integral must

satisfy the condition of making  $\sigma$  vanish for large values of  $r$ . This equation gives, by putting  $r=b$ , the pressure at any point of the sphere, from which the acceleration of the sphere by the total pressure might be calculated. It is not necessary, for the present purpose, to perform this calculation.

According to the above solution the absolute velocity of each fluid particle in contact with the sphere is directed from or towards the centre of the sphere, and is equal to  $T \cos \theta$ . Hence at points for which  $\theta = \frac{\pi}{2}$  the velocity is zero. This result stands in direct contradiction to the corresponding one of the former solution, according to which the velocity for the same value of  $\theta$  is at the surface of the sphere half that of the sphere itself and in the contrary direction. Also since at any point for which  $\theta = \frac{\pi}{2}$  the velocity is transverse to the same plane, and varies inversely as the cube of the distance from the centre of the sphere, it follows that at *each instant* just as much fluid is flowing backwards as in the direction of the sphere's motion, and that there is absolutely no transfer of the fluid by the impulse of the sphere. Certainly the condition of constancy of mass is satisfied by this result, the general equation expressing that condition having, in fact, formed an essential part of the basis of the reasoning. But when it is considered that the fluid is unlimited in extent, and that the impulse is propagated indefinitely into space, the result clearly involves an incompatibility which indicates that the premises of the reasoning are either false or insufficient. According to the views I am maintaining, they are insufficient.

A fixed point on the straight line in which the centre of the sphere moves being the origin of the rectangular coordinates  $x, y, z$ , and the axis of  $x$  coinciding with this line, let the plane containing that axis and any point  $xyz$  make an angle  $\eta$  with the plane of  $xy$ . We shall then have, at any time  $t$ ,

$$x = \int T dt + r \cos \theta, \quad y = r \sin \theta \cos \eta, \quad z = r \sin \theta \sin \eta,$$

$$u = \frac{Tb^2}{r^2} \cos^2 \theta, \quad v = \frac{Tb^2}{r^2} \cos \theta \sin \theta \cos \eta, \quad w = \frac{Tb^2}{r^2} \cos \theta \sin \theta \sin \eta,$$

and consequently

$$u dx + v dy + w dz = \frac{Tb^2}{r^2} \cos \theta dr.$$

It thus appears that for this motion  $u dx + v dy + w dz$  is not an exact differential, and that it may be made such by the factor  $\frac{1}{\cos \theta}$ . It has already been argued generally that that differential

is not necessarily exact because the motion is vibratory, and that a factor depending on the given circumstances of the motion always exists by which it may be made integrable.

I proceed next to the application of the same principles to the problem with which we are more immediately concerned, namely, that of determining the pressure at any point of the surface of a sphere at rest on which a series of waves is incident. Let the waves be defined by the equations

$$T = \kappa a \sigma_1 = m \sin \frac{2\pi}{\lambda} (\kappa at + x + c),$$

$x$  being reckoned from the centre of the sphere in the direction contrary to that of the propagation of the waves.  $T$  now expresses the velocity of the wave at any distance  $x$  from the origin at the time  $t$ ; and, so far as regards the reaction of the hemispherical surface on which the waves are incident, the fluid is *relatively* impressed just as in the preceding problem. Hence, if we neglect the small variation of  $\sigma_1$  at a given instant within the space over which the effect of the impression is sensible, we shall have, as before,

$$\kappa^2 a^2 \cdot \frac{d\sigma}{dr} + \frac{b^2 \cos \theta}{r^2} \cdot \frac{dT}{dt} = 0,$$

$r$  and  $\theta$  being polar coordinates referred to the fixed centre of the sphere. By integration along a line of impression, that is, along the prolongation of a radius of the sphere, we obtain

$$\kappa^2 a^2 (\sigma - \sigma_1) = \frac{b^2 \cos \theta}{r} \cdot \frac{dT}{dt},$$

the integral satisfying the condition that  $\sigma = \sigma_1$  for large values of  $r$ , and  $x$  being omitted in the value of  $T$ , as is allowable on account of the small magnitude of the sphere. The pressure due to the condensation at any point of the hemispherical surface is given by the equation

$$a^2 \sigma = a^2 \sigma_1 + \frac{b \cos \theta}{\kappa^2} \cdot \frac{dT}{dt},$$

whence the total pressure in the direction of the incidence of the waves, or  $2\pi b^2 \int a^2 \sigma \sin \theta \cos \theta d\theta$  from  $\theta = 0$  to  $\theta = \frac{\pi}{2}$ , will be found to be

$$\pi b^2 \cdot \left\{ \frac{Ta}{\kappa} + \frac{2b}{3\kappa^2} \cdot \frac{dT}{dt} \right\}.$$

The determination of the pressure on the other hemispherical surface is a problem of much greater difficulty, the solution of which I do not profess to have completely effected; but I con-



sider the following argument to be a step in advance of the researches that I have hitherto published upon it, and at least to suffice for the application I have in view at present. If the usual treatment of hydrodynamical questions be legitimate, the problem now under consideration presents no more difficulty than that of the vibrating sphere, the analysis to the first order of small quantities being precisely the same for the one as for the other. But that this cannot be true is evident from the consideration that the action on the fluid of that hemispherical surface of the fixed sphere on which the waves are incident differs essentially from that of the other, inasmuch as the latter does not directly impress any velocity on the fluid compelled to move in contact with it. The only arbitrary conditions which the part of the motion now under consideration is required to satisfy, are the compulsory movement along the surface of the sphere, and the state of the fluid as to velocity and condensation in the transverse plane passing through the centre of the sphere. By the results obtained above relative to the action of the first hemispherical surface the velocity is  $T$ , and the condensation  $\sigma_1$ , at every point of that plane at all times. In other respects the motion is determined by the mutual action of the parts of the fluid.

Let us now conceive the whole motion to consist of motions in an unlimited number of very slender tubular spaces, varying in position and transverse section, but always so that the axis of the tube coincides with the direction of the motion of the particles through which it passes. Let  $V$  be the velocity and  $\sigma$  the condensation at any point of one of these tubes at any time  $t$ . Then, from what has been already shown,

$$\kappa^2 a^2 \cdot \frac{(d\sigma)}{ds} + \frac{dV}{dt} = 0,$$

the line  $s$  being reckoned along the axis of the tubular space, and  $d\sigma$  in brackets signifying that the variation is from point to point of that line. Now since  $\sigma$  must be regarded as a function of  $x, y, z$ , and  $t$ , we have

$$\frac{(d\sigma)}{ds} = \frac{d\sigma}{dx} \cdot \frac{dx}{ds} + \frac{d\sigma}{dy} \cdot \frac{dy}{ds} + \frac{d\sigma}{dz} \cdot \frac{dz}{ds}.$$

Also

$$\frac{dx}{ds} = \frac{u}{V}, \quad \frac{dy}{ds} = \frac{v}{V}, \quad \frac{dz}{ds} = \frac{w}{V};$$

and since  $V^2 = u^2 + v^2 + w^2$ ,

$$\frac{dV}{dt} = \frac{u}{V} \cdot \frac{du}{dt} + \frac{v}{V} \cdot \frac{dv}{dt} + \frac{w}{V} \cdot \frac{dw}{dt}.$$

Consequently, by substituting in the foregoing equation,

$$\left(\kappa^2 a^2 \cdot \frac{d\sigma}{dx} + \frac{du}{dt}\right) \frac{u}{V} + \left(\kappa^2 a^2 \cdot \frac{d\sigma}{dy} + \frac{dv}{dt}\right) \frac{v}{V} + \left(\kappa^2 a^2 \cdot \frac{d\sigma}{dz} + \frac{dw}{dt}\right) \frac{w}{V} = 0.$$

As this equation has been obtained by perfectly general considerations, it is applicable to any instance of small motions. In the case of symmetrical motion relative to an axis, produced by a vibrating sphere, as also in that of the like motion due to the reaction of the first hemispherical surface of the fixed sphere, it was found that the motion was wholly in lines normal to the surface of the sphere, and the dynamical equation employed is that which the above equation becomes when the condition of central motion is satisfied. But in the present question there is no other condition than that the motion is symmetrical about an axis; in which case  $\sigma$ ,  $u$ ,  $v$ ,  $w$  are functions of the polar coordinates  $r$  and  $\theta$ . Let  $u$  and  $\omega$  be respectively the velocities parallel and perpendicular to the axis, so that  $\omega^2 = v^2 + w^2$ , and  $\frac{z}{y} = \frac{w}{v}$ . Taking

these equations into account, and transforming into polar coordinates, the last equation becomes

$$\left\{ \kappa^2 a^2 \cdot \left( \frac{d\sigma}{dr} \cos \theta - \frac{d\sigma}{d\theta} \cdot \frac{\sin \theta}{r} \right) + \frac{d}{dt} \right\} \cdot \frac{u}{V} + \left\{ \kappa^2 a^2 \cdot \left( \frac{d\sigma}{dr} \sin \theta + \frac{d\sigma}{d\theta} \cdot \frac{\cos \theta}{r} \right) + \frac{d\omega}{dt} \right\} \cdot \frac{\omega}{V} = 0.$$

Since no general relation exists between  $u$  and  $\omega$  other than that which results from the mutual action of the parts of the fluid, and since to find this relation is precisely what the analysis is required to do, we must equate separately to zero the multipliers of  $u$  and  $\omega$  in brackets. By combining the two equations thus obtained with the equation of constancy of mass, which, expressed in the same coordinates, is

$$\frac{d\sigma}{dt} + \frac{du}{dr} \cos \theta - \frac{du}{d\theta} \cdot \frac{\sin \theta}{r} + \frac{d\omega}{dr} \sin \theta + \frac{d\omega}{d\theta} \cdot \frac{\cos \theta}{r} + \frac{\omega}{r \sin \theta} = 0,$$

there results

$$\frac{1}{\kappa^2 a^2} \cdot \frac{d^2 \cdot \sigma r}{dt^2} = \frac{d^2 \cdot \sigma r}{dr^2} + \frac{1}{r^2} \left( \frac{d^2 \cdot \sigma r}{d\theta^2} + \frac{d \cdot \sigma r}{d\theta} \cot \theta \right).$$

This equation, after substituting  $-n^2 \sigma$  for  $\frac{d^2 \sigma}{dt^2}$ , differs in no respect from the one used in the first solution of the problem, excepting in having  $\kappa^2 a^2$  in the place of  $a^2$ . It applies, however,

only to motion due to the action of the second hemispherical surface.

Having brought the argument to this point, I reserve for the next communication the deduction of inferences from the above equation, and the application of results to the Theory of the Dispersion of Light.

Cambridge, May 18, 1864.

LXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 387.]

June 18, 1863.—Major-General Sabine, President, in the Chair.

THE following communications were read:—

“On the Influence of Temperature on the Electric Conducting-Power of Alloys.” By A. Matthiessen, F.R.S., and C. Vogt, Ph.D.

The subject of this paper has been divided into four parts, viz. :—

I. Experiments on the influence of temperature on the electric conducting-power of alloys composed of two metals.

II. Experiments on the influence of temperature on the electric conducting-power of some alloys composed of three metals.

III. On a method by which the conducting-power of a pure metal may be deduced from that of the impure one.

IV. Miscellaneous and general remarks.

In the first part, after having given the numerical results, we proceed to explain the law which regulates this property. It is as follows:—

*The observed percentage decrement in the conducting-power of an alloy between 0° and 100° C. is to that calculated between 0° and 100° C. as the observed conducting-power at 100° C. is to that calculated at 100° C.*

Or in symbols,  $P_o : P_c :: \lambda_{100^\circ} : \lambda'_{100^\circ}$

where  $P_o$  and  $P_c$  represent the observed and calculated percentage decrements in the conducting-power of the alloy between 0° and 100° C.; and  $\lambda_{100^\circ}$  and  $\lambda'_{100^\circ}$  its observed and calculated conducting-power at 100° C.,  $P_c$  is equal in nearly all cases to 29·307\*, the exceptions being only in the instances of thallium and iron alloys†.

The above law holds good for most of the alloys belonging to the first and third groups, as well as for a part of those belonging to the second group‡.

Now, if the above proportion,

$$P_o : P_c :: \lambda_{100^\circ} : \lambda'_{100^\circ} \dots \dots \dots (1)$$

be converted into terms of resistance, the following formula is obtained,

$$r_{100^\circ} - r_{0^\circ} = r'_{100^\circ} - r'_{0^\circ} \dots \dots \dots (2)$$

where  $r_{100^\circ}$ ,  $r_{0^\circ}$ ,  $r'_{100^\circ}$  and  $r'_{0^\circ}$  represent the observed and calculated

\* Phil. Trans. 1862.

† Phil. Mag. S. 4. vol. xxvi. p. 542.

‡ Phil. Trans. 1860, p. 161.





We verify this by comparing the conducting-power of a pure metal directly determined, with the conducting-power of the same metal deduced from a determination of the conducting-power of its alloy with small quantities of other metals. It is a curious fact, that the deduced values from experiments upon hard-drawn wires are in reality the conducting-powers of the annealed wire of the pure metal. After having thus verified the method, we have not hesitated to employ it in the determination of the conducting-power of certain metals which have not yet been experimented upon in a state of purity.

In the fourth part we point out, first, that the percentage decrement in the conducting-power of alloys between  $0^\circ$  and  $100^\circ$  is never greater than that of the pure metals composing them; secondly, that the conducting-power of alloys decreases with an increase of temperature (some bismuth alloys form an exception to this law); thirdly, that in some cases the percentage composition of an alloy may be deduced from its conducting-power, with the aid of the percentage decrement in its conducting-power; fourthly, the method which we have used for determining the class to which the metals belong in respect to the conducting-power of their alloys; and fifthly, that the results which we have obtained and described in this memoir fully bear out the views put forward in a former one on the chemical nature of alloys.

“On the Peroxides of the Radicals of the Organic Acids.” By Sir B. C. Brodie, Bart.

In a former notice published in the *Phil. Mag.* S. 4. vol. xvii. p. 301, an announcement was made of the discovery of a new group of organic combinations, the peroxides of the radicals of the organic acids—bodies which in the systems of the combinations of these radicals occupy the same relative position as is held by the peroxides of hydrogen, barium, or manganese in the systems of the combinations of those elements. An account was given of the mode of preparation and properties of two members of this group, the peroxides of benzoyle and of acetylene,  $C_{14}H_{10}O_4$  and  $C_4H_6O_4$ . The present paper contains an extension of this inquiry. In it is given an account of several other peroxides of monatomic radicals, the peroxides of nitro-benzoyle, of cumenyle, of butyle, and of valeryle, and also an inquiry into the action of the peroxide of barium on the bibasic anhydrides.

The nitro-benzoic peroxide is formed by the action of fuming nitric acid on the peroxide of benzoyle. It stands to peroxide of benzoyle in the same relation as anhydrous nitro-benzoic acid stands to anhydrous benzoic acid, and may be regarded as derived from that peroxide by the substitution in it of two atoms of peroxide of nitrogen for two of hydrogen. The formula of the substance is  $C_{14}H_9(NO_2)_2O_4$ .

			Calculated.	Found.
$C_{14}$	168	.....	50.60	50.60
$H_9$	8	.....	2.41	2.58
$N_2$	28	.....	8.43	8.49
$O_8$	128	.....	38.56	38.33
	332		100.00	100.00

The peroxide of cumenyle is procured by a process strictly analogous to that by which the peroxide of benzoyle is formed; it has the constitution  $C_{20}H_{22}O_4$ .

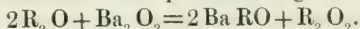
The peroxides of butyle and valeryle are prepared by the action of hydrated peroxide of barium on the anhydrous acid. It is only necessary to mix in a mortar equivalent quantities of the two substances. The peroxide is separated by solution in ether from the water in which it is dissolved and suspended. These substances are dense oily fluids, exploding slightly when heated, but not so readily decomposable as the peroxide of acetyle. The analysis of the peroxide of butyle, dried by chloride of calcium, gave results corresponding with the formula  $C_8H_{14}O_4$ .

		Calculated.	Found.
$C_8$	96	55.17	55.11
$H_{14}$	14	8.05	8.28
$O_4$	64	36.78	36.61
	<u>174</u>	<u>100.00</u>	<u>100.00</u>

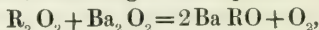
The analysis of the peroxide of valeryle gave results corresponding with the formula  $C_{10}H_{18}O_4$ .

		Calculated.	Found.
$C_{10}$	120	59.40	59.39
$H_{18}$	18	8.91	9.17
$O_4$	64	31.69	31.44
	<u>202</u>	<u>100.00</u>	<u>100.00</u>

The mode of formation of these peroxides is given in the equation



These substances are decomposed as well as formed by the action of the alkaline peroxide, according to the equation



giving a striking example of those consecutive actions referred to in a former paper as the cause of certain catalytic decompositions.

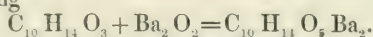
The action of the bibasic anhydrides on the alkaline peroxides is of special interest.

When anhydrous succinic acid, lactide, or anhydrous camphoric acid is mixed with an equivalent of hydrated peroxide of barium, a solution is obtained possessing the most powerful oxidizing properties, which bleaches indigo, evolves chlorine with hydrochloric acid, and oxidizes the protosalts of iron and manganese, but which does not discolour permanganic acid, or give with chromic acid the blue colour formed by peroxide of hydrogen. When boiled, the solutions evolve oxygen, and afterwards contain a salt of the acid employed—in the case of succinic acid, giving a crystalline precipitate of succinate of barium, and in the case of camphoric acid, giving with acetate of lead a precipitate of camphorate of lead. These solutions are in a state of continual decomposition. Only in one instance, that of camphoric acid, was it found possible to analyse the substance, and that only by indirect processes. The oxygen contained in the organic peroxide was estimated in a measured portion of the solution by means of a standard solution of iodine; the camphoric acid formed on boiling was determined by precipitation with acetate of lead in another

measured portion; and in a third portion the barium was estimated as sulphate. The results of these determinations are given below, the camphoric acid being assumed as correct. They lead to the conclusion that the solution contains the elements of one equivalent of anhydrous camphoric acid, one of oxygen, and one of baryta.

	Atomic weight.	Calculated ratio.	Found.
$C_{10}H_{14}O_3$ .....	182 .....	25·12 .....	25·12
$O$ .....	16 .....	2·20 .....	2·07
$Ba_2O$ .....	153 .....	21·12 .....	21·51

the reaction being



That the substance formed is to be regarded as the baryta salt of the peroxide of camphoryle, and not as the camphorate of the peroxide of barium, is proved by the reactions of the solution, which does not give peroxide of hydrogen when decomposed by acids, or a precipitate of the hydrated peroxide of barium when heated with a solution of baryta.

The organic peroxides constitute a new and peculiar group of chemical substances characterized by reactions never hitherto found in any compound of carbon, and which materially extend our views of the possible properties of the so-called organic combinations, and of their analogies to inorganic substances. They are the organic representatives of chlorine in the same sense as the oxides of the compound ammoniums are the representatives of potash, and in a yet closer sense than ether and alcohol resemble the oxide and its hydrate, or than ethyle or marsh-gas are analogous to hydrogen. This analogy is of a profound character, not consisting merely in the analogy of symbolic form, but in the absolute identity of reactions. The admitted analogies of the peroxide of chlorine have as it were their maximum in the organic peroxide. Not only is chlorine represented in the peroxide, but hydrochloric acid is represented in the organic acid, and a series of parallel equations may readily be constructed, showing the identical character of the reactions of the two classes of substances. Both bleach a solution of indigo, oxidize the protosalts of iron and manganese, decompose water under the influence of sunlight, and evolve oxygen with an alkaline peroxide, forming the salt of the corresponding acid.

“On the Magnetic Disturbance which took place on the 14th of December 1862.” By Balfour Stewart, M.A., F.R.S.

On the 14th of December 1862, a magnetic disturbance occurred about 6 o'clock in the afternoon, and was registered by means of the Kew magnetographs. As usual it was accompanied by an auroral display and by earth-currents, and the latter phenomena were observed at Greenwich by means of a system of telegraphic wires which had recently come into the possession of the Astronomer Royal.

By the kindness of Mr. Airy, the Kew Observatory has been favoured with a copy of the curves which represent the earth-currents collected at Greenwich during the progress of this disturbance; and a comparison of these with the Kew magnetograph curves will form the subject of the following paper. It will, however, first be



necessary to state the arrangement adopted at Greenwich. There are two wires proceeding from the Observatory, one ending near Croydon and the other near Dartford (nearly at right angles); and these are well insulated throughout their entire length, and have good earth-connexions at their extremities.

From these, by means of galvanometers, the intensity of earth-currents is recorded for two very favourable directions; and from these again the intensity of those currents which flow in the magnetic meridian, and in a direction perpendicular to it, may be very easily deduced. The standard for direction is the current which flows through the wire of a battery from the zinc pole, and which is called the zinc-current. With this explanation the earth-current diagrams appended to this paper will be quite intelligible; and with regard to the magnetic curves, it is only needful to remark that increasing ordinates denote decreasing declination and decreasing horizontal force, and that the normal lines, which have been furnished through the kindness of General Sabine, denote the position which the curves would have occupied had no disturbance supervened. But before proceeding to compare together the two sets of curves, it will be necessary to advert to a peculiarity of disturbances which enters as an essential element into all such discussions. It has been found by General Sabine that if the disturbances of declination be divided into two categories, easterly and westerly, these obey very different laws of daily variation, and also that this difference is not of the same description for all stations; so that we are compelled to view a magnetic disturbance as the resultant effect of two disturbances of different character, superimposed upon one another. General Sabine has likewise stated his opinion that this duality of action may perhaps be due to the disturbing force entering the earth at two or more points, one denoting magnetism of a more permanent kind, and the other magnetism of an induced description. A study of the Kew disturbance-curves tends to give confirmation to such an idea; for in these it is seldom found that the whole body of force which produces a disturbance is one which preserves the same type throughout and only varies in intensity. Even if we suppose that this type will vary with the hour of the day, we shall find, if we take disturbances which last for several days, that the type of force at a given hour of the first day is in very many cases different from that during the same hour of the second. When, however, we confine our attention to very abrupt changes of force, we find that the disturbance-type which these display retains more of the same character throughout a disturbance. In order to explain this, we may perhaps suppose that there are two sets of magnetic particles in the earth—one set being of the nature of soft iron, and the other similar to hardened steel. Now only the first of these would be acted on by any very sudden change in the disturbing force, since it would require time in order to influence the second set. We may thus perhaps account for the fact that any very sudden change is of one type, since it only influences one set of particles. Let us now consider what will take place if a disturbance of the same primary nature continues for any length of time. Here the hard-iron particles will also be influenced



to an extent compounded of the time and of the average value of the disturbing force during that time. The magnetic needle, therefore, will now be acted on by the joint influence of these two sets of particles, whereas at the beginning of the disturbance it was only acted on by one of them, namely, the soft-iron ones. The type of force will therefore have changed if the hard-iron particles are differently distributed in our globe from the soft-iron ones; and if, instead of two, there are many sets of particles, we shall have a very complicated effect.

Now this duality of disturbing forces must be considered when we attempt to ascertain the connexion between such forces and earth-currents, since we are not entitled to suppose that one of these two forces is related to earth-currents in precisely the same manner as the other. A small soft-iron force may be comparable to a large hard-iron one as far as an earth-current is concerned; or the reverse may be the case. When, however, there is a very rapid change of disturbing force, since this affects the earth through only one set of particles, we shall by its means be better able to trace the bond of connexion between a single type of magnetic force and the corresponding earth-current. Rapid changes of force are therefore of peculiar value in such an investigation.

Bearing this in mind, let us endeavour to connect together the phenomena of earth-currents and magnetic disturbances by two successive hypotheses, one of which, it would seem, must represent the truth unless there be some new link of connexion between magnetism and electricity with which we are unacquainted. The first hypothesis is that in which earth-currents are supposed to give rise to magnetic disturbances according to the laws by which a current acts upon a magnet. This may be called the theory of direct action.

By the second hypothesis, earth-currents are supposed to be induced, or secondary currents generated in the crust of the earth by those small but rapid changes in terrestrial magnetism which constitute disturbances. This may be called the theory of induced action.

Now, first, on the theory of direct action. Zinc-currents going to the south should correspond with magnetic disturbances decreasing the declination; and zinc-currents going to the east should correspond with an increase of horizontal force; but we find by the diagrams that while on this hypothesis the disturbance of the horizontal-force needle will be tolerably well accounted for by the east and west currents, the same correspondence does not hold between the north and south currents and the declination-disturbance. But I do not think that this circumstance, rightly viewed, tells either in favour of or against the hypothesis. A glance at the earth-current curves will show that the ordinates of the one bear generally a fixed proportion to those of the other, showing us that the total current has flowed backwards and forwards along one line\*; and though it is equally apparent, by a glance at the magnetic curves, that the same type of force has not been preserved throughout the disturbance, yet the explanation of the unity of type in the earth-currents may be that these are twisted into a line of motion, owing to the disposition of the conduct-

\* This had been previously observed in other cases by Mr. C. V. Walker.

ing strata of the earth's surface, just as a current can only move backwards or forwards along an insulated wire. Indeed a little reflection will show us that earth-currents are not local phenomena; so that if we endeavour to estimate quantitatively their influence on the magnet according to the hypothesis of direct action, we must first extend our field of observation, and obtain their value in other countries besides our own.

But to return to our comparison of curves. We see that for the greater part of the disturbance both the horizontal force and the declination were very much above their normal lines, while on the other hand the currents were frequently crossing their zero lines,—and that both currents were simultaneously and for a long portion of their time very near zero, although during this time the magnetic disturbance was considerable.

Next, with respect to a very abrupt disturbance which commenced about 8<sup>h</sup> 50<sup>m</sup>, the corresponding earth-current curves are exceedingly powerful, alternately passing and repassing the zero line to nearly the same distance on both sides, while the absolute disturbance of the horizontal force, and probably of the declination, was not very great.

We have thus, in the first place, a very sluggish action of earth-currents, while the magnetic disturbance was considerable, and in the next place a very violent action of the former when the absolute disturbing force was by no means excessive; and we may add that at 9<sup>h</sup> 50<sup>m</sup> both earth-currents were near zero, while both elements of the earth's magnetism were much disturbed.

For all these reasons this comparison of the curves is unfavourable to the hypothesis of direct action.

Let us now consider the other hypothesis, or that of induced action, and we shall find the following points in its favour.

1st. That in this disturbance, for at least one hour, both elements of the earth's magnetic force remained at a considerable distance from their normals, and that during this time the earth-currents observed were exceedingly small. Now, on the hypothesis of induced action, the earth-current effect depends not on the absolute value of the disturbing force, but on its rate of change; and if during this period we examine the horizontal-force curve, we shall find the abruptness of change not so great as in those cases where greater earth-currents were produced, while in the declination-curve the abruptness of change during this period is exceedingly small.

2nd. A reference to the diagrams will show us that in general the most abrupt magnetic disturbances are those which are accompanied by the greatest earth-currents, and that in particular a very abrupt disturbance, which took place about 8<sup>h</sup> 50<sup>m</sup>, was accompanied by very strong earth-currents, alternately positive and negative, those of the one name being nearly as powerful as those of the other, while on the other hand the corresponding magnetic disturbances were on an average decidedly on one side of the normal lines.

On the other hand, the following fact seems at first to tell against the theory of induction. An inspection of the curves will show that we have currents remaining on one side of the zero-line for some length of time, during which the magnetic disturbances have never-

theless changed in both directions. When, however, we reflect on this circumstance, we are led to see that since we have two sets of disturbances taking place simultaneously, so we must also have two sets of earth-currents. Now one of these disturbances, which we may perhaps call the soft-iron one, reproduces those small and rapid changes which take place in the primary force, while on the other hand the hard-iron disturbance averages these small changes and presents us with a disturbance-wave of long period. Precisely, then, as in the magnetic curves we have waves of short period superimposed upon waves of long period, so will it be in the earth-current curves. Those currents due to the soft-iron disturbances will be superimposed upon those due to the hard-iron ones, with this difference, that we are not entitled to assume that the proportion in intensity between the two simultaneous earth-currents must be precisely that which exists between the rates of change of the two corresponding simultaneous disturbances. It will be apparent that this feature of duality ought also to be presented by the aurora; and here it is well known that we have at least two phenomena, one of a more fitful and the other of a more permanent character, namely, the streamers and the auroral arch. We may suppose the first of these phenomena to correspond to the soft-iron, and the second to the hard-iron disturbances. Indeed it is questionable whether the different varieties of auroræ are confined to these two; for General Sabine has informed me that he himself, along with the late Sir Edward Parry, observed at Lerwick in the Shetland Isles in 1818, at the same instant, two auroral arches crossing one another at an angle. But, be this as it may, when we reflect that there are many kinds of particles in our earth, some of which may be affected more rapidly than others by a primary magnetic force, we shall cease to wonder that the phenomena presented are of a complicated description.

All these considerations have induced me to think that it is lost labour to attempt a quantitative comparison when our observation of the magnetic disturbances and their corresponding earth-currents is confined to one locality; and it will be seen from this paper, that while endeavouring to uphold the hypothesis of induced action, I have done so by a comparison of a general and qualitative rather than by one of a quantitative nature.

“On the Change in the Elastic Force of a Constant Volume of dry Atmospheric Air, between  $32^{\circ}$  F. and  $212^{\circ}$  F., and on the Temperature of the Freezing-point of Mercury.” By Balfour Stewart, M.A., F.R.S.

The author gave a detailed description of his apparatus, and of the method employed in drying the air.

The result of his experiments gave as the coefficient of increase of elasticity of air of constant volume for  $1^{\circ}$  F. 0.002040, this being slightly different from that given by Regnault, which is 0.002036.

He also finds that the temperature of the freezing-point of mercury is constant, and that its value on Fahrenheit's scale, as given by an air-thermometer, is  $-37^{\circ}.93$ , while as recorded by a standard mercurial thermometer it is  $-38^{\circ}.00$ . It is probable that this difference is owing to an anomalous contraction of mercury before it freezes, similar to the corresponding expansion of water; but this effect in the



case of mercury seems to be very small, and it may be said that a mercurial thermometer properly graduated, will denote the true temperature, even down to the freezing-point of the mercury itself.

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GEOLOGICAL SOCIETY.

Continued from p. 156.]

January 20, 1864.—Professor A. C. Ramsay, President,  
in the Chair.

The following communications were read :—

1. "Observations on supposed Glacial Drift in the Labrador Peninsula, Western Canada, and on the South Branch of the Saskatchewan." By Professor H. Y. Hind, M.A., F.R.G.S., Trinity College, Toronto.

During an exploration of a part of the interior of the Labrador peninsula, in 1861, the author had an opportunity of observing the magnitude, distribution, and extraordinary number of the boulders on the flanks of the tableland of that area; and he commenced this paper with a detailed account of the results of his observations, referring also to the forced arrangement of blocks of limestone, shale, and Laurentian rocks in Boulder-clay at Toronto, and on the south branch of the Saskatchewan.

Professor Hind then described briefly the Driftless Area, in Wisconsin, discovered by Prof. J. D. Whitney, and the conclusions to which that geologist has been led by the study of this district. He next adverted to the beaches and terraces about the great Lakes, and considered their origin to be similar to that suggested by Mr. Jamieson for the Parallel Roads of Glen Roy. The formation of anchor-ice in the Gulf of St. Lawrence and at the heads of rapids in the great river itself was alluded to as one of the means by which river-beds may be excavated. The parallelism of escarpments in America, at great distances apart, and at elevations varying from 600 feet to 3000 feet above the sea, was next described, and their symmetrical arrangement suggested to be the result of glacial rivers undermining the soft strata of sedimentary rocks in advance of the glacial mass itself. These escarpments were also thought to represent different and closely succeeding glacial epochs.

2. "Notes on the Drift-deposits of the Valley of the Severn, in the neighbourhood of Coalbrookdale and Bridgenorth." By George Maw, Esq., F.S.A., F.L.S.

The patches of Drift occurring in the Valley of the Severn from about four miles below Bridgenorth up to Shrewsbury, including a north and south range of about twenty miles, have been carefully examined by the author, and were described in detail in this paper.

Commencing with Strethill, a hill close to the entrance of Coalbrookdale, the author described the several beds which make up the Drift-deposits of which it is composed, and gave a list of the rocks which he had found in them. In the same manner he described in succession the neighbouring districts in which the Drift-deposits are exhibited, and gave a list of the fossils which had been found in the beds at the different localities.



In conclusion Mr. Maw put forward some hypotheses as to the period when the degradation of the older formations (the materials of which compose the Drift) took place, the manner in which the Drift was deposited, the extent of the submergence of England and Wales during the period of its deposition, and the influence of glaciers and glacier-action in its production.

*LXXII. Intelligence and Miscellaneous Articles.*

ON THE DYNAMICAL THEORY OF HEAT.

*To John Tyndall, Esq., F.R.S., &c.*

SIR,

I AGREE with the Editors of the Philosophical Magazine, that it is not desirable that my remarks on the dynamical theory should give rise to prolonged discussion. What is wanted is experimental proof of the direct conversion of heat into mechanical work; and after what has been so ably achieved by Dr. Joule for physical science, in proving beyond a doubt the conversion of work into heat, and determining the numerical equivalent of the transformation, surely it is reasonable to expect that every effort should be made to obtain positive physical evidence of the reality of the conversion of heat into work. I thank the Editors for kindly allowing me to state the difficulties which I have encountered in this pursuit, and the consequent doubts which have presented themselves of the completeness of the dynamical theory as it now stands; and I beg to avail myself further of their courtesy to clear up in a few lines some want of perspicuity in my last letter, which may in some degree have influenced the remarks of Dr. Rankine and of Mr. Croll in the Philosophical Magazine for March.

Dr. Rankine is right in pointing out my inadvertence in overlooking the application of the second law of the action of heat in two instances. In the first, namely, my statement that the regenerator might take up and restore all the heat remaining in air after it has done work by expansion, my omission was but half unconscious, and arose from my having uppermost in my mind the ideas of thermodynamic action published by me some years ago; and had I entered more into detail on this point in my last letter it would have been seen that my oversight was more in appearance than in reality, as the difficulty of the hypothetical case proposed by me would not have been removed by a formal application of the second law. In the second case, the estimate I gave of the useful effect of steam-engines was the old statement of M. Regnault; and I should have stated that it was quoted by me from M. Verdet's Second Lecture on the Thermo-dynamical Theory of Heat (Paris, 15th February 1862, section 2). Of course, in this case the application of the second law made it requisite to suppose (in addition to M. Regnault's deductions from the mere calculation of the latent heat) the condensation of some steam as a result of the actual conversion of heat into work; but as this is the very point at issue, and a point on which the grand question of the practical improvement of our heat engines mainly depends, I may be allowed to state that, as far back

as 1843, I published results of my experiments, showing that the whole heat resulting from the condensation of steam in the cylinder of an engine working expansively should be found in the condenser, after allowing for known losses from other causes—a result which subsequent experience has steadily confirmed.

I allow the propriety of Dr. Rankine's disapprobation of my remark, that M. Hirn was borne down by scientific authority in altering his opinions on the results of his own experiments, as, after all, the publication of this change of opinion was his own free act. Though I said "apparently borne down," the expression was inconsiderate, and I freely retract it. I cannot forget that I am indebted to the courteous kindness of Dr. Rankine for spontaneously sending me valuable scientific information, which in my comparatively isolated position here I could not otherwise have easily obtained; and though I reluctantly differ from him on some points of scientific theory, my high appreciation of his intellectual superiority will always cause me to respect and value his opinion, whether for censure or approbation.

While thanking Mr. Croll for his remarks on my uncertainty about the estimate of work done by heated air in the shape of raising the weight of the atmosphere, and on the specific heat of air, I would beg to refer back to my chief difficulty on this latter point, which perhaps I did not state with sufficient clearness, viz. the disappearance of energy in the supposed experiment. What I meant to express was this:—There must be more heat in the air after expansion than before, because the energy which might have been availed of by allowing the atmosphere to fall back into the vacuum, and thus produce work, has disappeared, and we can imagine no other equivalent for it. And the specific heat of the expanded air, or its capacity for heat, is greater because it does not show this increase of quantitative heat.

Not having been able to make the result of experiment agree with the dynamical theory as to the direct conversion of heat into work, I have been long pondering on some consistent explanation of the physical facts, which have continued to present themselves in my researches. On this subject I had a thing to say, but let it go for the present, and now I will return with renewed energy to the experimental investigation of this very interesting but still perplexing subject.

Believe me, Sir, your most obedient Servant,

Palermo, April 4, 1864.

JOSEPH GILL.

#### ON THE HEAT OF THE SUN'S RAYS. BY M. HAGEN.

M. Hagen made a communication to the Berlin Academy on the Heat of the Sun's Rays; he had taken some observations made in Madeira by the late Dr. Hagen as a basis.

The most important results were—

1. That the height of the atmosphere, assuming that the layers of air have the same power of absorption, is only equal to the 173rd part of the earth's radius.
2. The amount of the heating effect possessed by the sun's rays

on entering this atmosphere may be expressed by saying that a bundle of rays having a section of a square inch would in one minute raise the temperature of a cubic inch of water by 0·733 of a degree Centigrade.

3. The loss of heat in the passage of the rays through the atmosphere, according to measurements made on different days and in different seasons, with apparently clear days, diminishes by amounts which materially differ from one another. The logarithms of the factors which denote the absorption on a path the length of the earth's radius vary between  $-3$  and  $-38$ .

These results were finally compared with those of Pouillet, and it was found that for the factor mentioned, the latter denote indeed much narrower limits, but still values which coincide with those developed here. Pouillet, on the other hand, found the heat of the sun's rays to be one-eighth less; and this difference appears to be explained by the fact that the height of the atmosphere was taken too high, and put equal to the eightieth part of the earth's radius.—*Berichte der Berliner Akademie*, November 1863.

STATE OF METEORIC SCIENCE. BY A. S. HERSCHEL, ESQ.

It is a saying of Arago, founded originally upon observation, and confirmed by constant experience in later years, that the earth encounters more shooting-stars in going from aphelion to perihelion than in going from perihelion to aphelion. In point of fact, between the north latitudes of  $49^{\circ}5$  and  $54^{\circ}2$ , Dr. Julius Schmidt, the indefatigable Director of the Observatory at Athens, observed, during eight years, from 1843 to 1850, on an average 470 meteors in every year. His observations were distributed in the several months, according to the following average of the entire series:—

Anomalistic Year.	Month.	Shooting-stars.	Total Shooting-stars.
Aphelion to Perihelion	July .. . . .	49	400
	August .. . . .	188	
	September . . .	41	
	October . . . . .	37	
	November . . . .	54	
	December . . . .	31	
Perihelion to Aphelion	January . . . . .	17	70
	February . . . .	5	
	March . . . . .	11	
	April . . . . .	11	
	May . . . . .	12	
	June . . . . .	14	
Total . . . . .		470	

Supposing, with Arago, that shooting-stars are meteorites or particles describing planetary orbits in all directions round the sun, no one of these meteorites can overtake the earth, if the native velocity of all is less than the planetary motion of the earth itself; on the contrary, the earth will overtake, in a greater or less degree, the whole of those which it meets, if we suppose its velocity to be greater than that of every individual shooting-star. Even upon



more general assumptions, however, of the orbital velocities of meteorites, it follows that a more or less powerfully determined current of falling stars must arrive upon the earth, owing to its planetary motion, from a constantly prevailing direction ninety degrees in longitude behind the sun.

On this account a season of frequency of aërolites, shooting-stars, and bolides must be expected to succeed, in all latitudes, three months later than the summer season of the sun; but, on the other hand, a dearth of meteors, in the spring, one quarter of a year later than midwinter. In general, and in all latitudes, the meteoric seasons, or seasons of meteoric frequency, must strictly follow the tropical seasons, and three months later in the year. Thus in the earth's northern hemisphere the northern pole remains directed to the sun from the equinox of March until that of September, and to the course of meteors from the solstice of June to the solstice of December. The greatest frequency of the meteorites will fall about the equinox of autumn, in September and October. This most nearly agrees with European observations. The meteoric season of Arago may, therefore, be drawn as a consequence from his planetary hypothesis, if it be permitted to change the limits which he assigns to it by a small quantity, namely, from the earth's apsides to its solstices in its orbit.

The same fact, which appears strongly marked with regard to shooting-stars in the eight-year summary of Dr. Schmidt, is found repeated in a striking manner in the existing Northern Catalogues of *Star-showers*, *Fire-balls*, and *Aëroliths*. The following references may be taken as examples:—

Appearances.	Number, July to December.	Number, January to June.
Star-showers, from 1800 B.C. In M. Quetelet's Catalogue ( <i>Physique du Globe</i> , 1861).....	72	28
Aërolitic meteors, from the Christian era. In Mr. Greg's Catalogue (Brit. Assoc. Report, 1860).....	216	186½
Large and small fire-balls. In same Catalogue ( <i>Ibid.</i> ) .....	843	553

The time appears therefore at length to have arrived when observations of meteors in the southern hemisphere may greatly contribute to establish the *planetary* theory of meteorites. Should the same sequences of meteoric seasons, after the tropical seasons, be observed in the southern hemisphere which are followed in the north, the record of meteors in southern observatories will become at once a confirmation of the planetary motion of the earth, and a proof of the correctness of the existing theory of shooting-stars. A sufficient number of data can only be collected in the course of time; but the observations of shooting-stars at Melbourne, in South Australia, already preserved for some years, in great numbers, by Dr. Neumayer, appear actually calculated to give to this question, before long, a satisfactory solution.—*From the Monthly Notices of the Royal Astronomical Society*, April 8, 1864.



THE  
LONDON, EDINBURGH AND DUBLIN  
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AND  
JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XXVII. FOURTH SERIES.

LXXIII. *On the Origin of the Swiss Lakes.*

By Professor B. STUDER of Berne\*.

WHEN we reflect upon the origin of the Swiss lakes, we find that it constitutes a problem very difficult of solution, and it is not easy to determine, in the series of geological events, the moment of the formation of the basins which contain them. Eminent geologists, both of Switzerland and of foreign countries, have proposed solutions of this problem; all the forces of nature have been set in action, and nevertheless we appear to be further than ever from its settlement. To be convinced of this, we have only to glance at the excellent summary of the various opinions and of the present state of the question which has just been published by M. de Mortillet†.

The authors may be divided into two classes. Some, amongst whom we find von Buch‡, F. Hoffman§, and Ball|| (according to his remarkable memoir lately published), believe that the same causes which have convulsed the ground in elevating our mountains, have also produced the depressions which separate them. They think that the elevation was accompanied by crevasses of greater or less depth, which have formed our valleys, and that empty spaces were left in the interior, of which the roof has subsequently given way. Lastly, they believe that the basins of our lakes are the remains of these crevasses not yet filled up by the detritus brought down by the rivers. The others, disciples of Buffon, Playfair, and the Wernerian school, ascribe the origin of the valleys and basins to erosion, that is to say, to the destructive action of fluids in motion. These partisans of erosion have quite lately separated again into two classes, being unable to come to an agreement as to the nature of the destruc-

\* Translated by W. S. Dallas, F.L.S., from the *Bibliothèque Universelle*, 1864; *Archives des Sciences*, p. 89.

† *Atti della Soc. Ital.* November 1863.

‡ *Catal. des Roches de Neuch.* (1804) MS.

§ *Physic. Geogr.* (1837).

|| *Phil. Mag.* February 1863.

tive fluid. Some, following their masters, admit only marine currents, rivers, or torrents; the others, amongst whom we find some justly celebrated English and Italian physicists and geologists, have lately proposed to accept the intervention of the erosive action of glaciers.

Each of these views is justified by the facts which take place before our eyes, for nature frequently makes use of very different means of producing the same effect. Desor, in an excellent article "On the Physiognomy of the Swiss Lakes"\* , adopts both the principal theories, and applies them according to the nature of the lake the origin of which he is endeavouring to explain. He distinguishes the *orographic lakes*, or those depending on the orography of the country, from the *lakes of erosion*, of which the basins have been hollowed out by water. The former are divided into three categories:—1st, the lakes of the synclinal or boat-shaped valleys†, such as the Lac du Bourget, the Lac de Joux, and the Lac de Saint-Point; 2nd, the lakes of the isoclinal or combe-like valleys, among which are the lakes of Brienz and Wallenstadt; and 3rd, the lakes of the transverse valleys or *cluses*, of which the lakes of Thun and Uri are examples. The alpine lakes, according to M. Desor, are chiefly orographic lakes; whilst those of Neuchâtel, Bienne, Morat, Zurich, and Constance, and others situated in the lower parts of Switzerland, are lakes of erosion. Some are the combined product of both principles; the Lake of Geneva, from Villeneuve to Vevey, is a transverse-valley lake (*lac de cluse*), and from Vevey to Geneva a lake of erosion. In a supplement‡, M. Desor extends his classification to the lakes of the Italian slope of the Alps, where he finds, according to M. de Mortillet§, a new category in the *lakes of moraines*. Their origin is explained by ancient moraines, which have served as barriers to the water at the mouth of the valleys.

The question becomes still more complicated if, in order to determine the epoch of formation of the lakes, we examine the geology of the strata which surround them. All the lower part of Switzerland and of the first chain of the Jura is sprinkled over with Alpine blocks, which, whatever may have been their mode of transport, must necessarily have passed over the lakes in order to pass from their original position to their present place, and we cannot conceive but that the current which carried them would have filled up these basins and formed a great cone of débris

\* *Revue Suisse* (1860).

† M. Desor calls these valleys *vallons*, but this term does not appear to be well chosen; *vallon* is the diminutive of *vallée*, and the synclinal valleys, on the contrary, are the largest in the Jura.

‡ *Actes de la Soc. de Lugano* (1861).

§ *Bull. de la Soc. Géol. N. S.* vol. xvi.

at the opening out of the Alpine valleys. This difficulty, against which the genius of von Buch, De Luc, Escher, and others had to struggle during the first and greater part of the present century, has been one of the principal causes of the readiness with which the hypothesis of the great extension of the ancient glaciers has been welcomed. In this theory, the Alpine blocks have been transported over the depressions between the Alps and the Jura, solidly supported upon ice, instead of being suspended in the air or in water at several thousand feet above the ground. This general coat of ice, which covered all the valleys and all the bottoms, allowed the epoch of the formation of the lakes to remain undecided. They might have been anterior to the glacial epoch, their basins during this period being filled with water or ice; and no one thought it necessary to assume that they were posterior to it, so recent an origin appearing irreconcilable with the evident connexion between these basins and the orography of the country.

For a long time, however, there has been known beneath the erratic stratum which accompanies the blocks, a bed of gravel and sand horizontally stratified, and possessing all the characters of a river deposit. This formation, called the *old transported bed* (*terrain de transport ancien*) by Elie de Beaumont, *old alluvium* by Necker, and *diluvium* by recent authors\*, is readily distinguished from the erratic stratum which is superimposed upon it. The latter contains a disorderly collection of blocks of all dimensions, and pebbles, frequently striated, in a peculiar mud which is never stratified; whilst in the lower stratum beds or long lenticular accumulations of smooth pebbles, nearly of equal size, alternate or interlock with lenticular or irregular masses of sand, the gravel and sand being either moveable or agglutinated. On examining this gravel of the old alluvium, it is easily seen that its pebbles are, without exception, derived from the Alps or the Subalpine ridges. Like the erratic blocks, they present different characters according to the valley through which they appear to have been conveyed. They correspond with the rocks in position in this valley and its tributary valleys. It is evident that the presence of this ancient alluvium throws us back again into all the difficulties from which we believed ourselves to have

\* This term should only be applied to the erratic stratum, and it would be better to suppress it altogether. It was introduced, I believe, by Buckland, to designate some recent strata which appeared to him to owe their origin "to a violent and transient flood," and soon afterwards it was extended to nearly the whole of the quaternary strata. I do not know when or by whom this name was applied, in opposition to its etymology and in contradiction to its original signification, to the stratum of ancient alluvium, which bears all the characters of a tranquil deposit from running waters acting through a long series of years.



escaped by means of the hypothesis respecting the extension of the ancient glaciers; and this hypothesis in this case can no longer render us the same service.

The difficulty may be diminished by reducing as much as possible the mass of the gravels the transportation of which across the lakes, before the great extension of the glaciers, appears to be inevitable. Indeed we know stratified quaternary gravels scarcely distinct from each other in their composition, but which evidently belong to very different ages. In the environs of Lyons and Vienne there are incoherent gravels, identical with some old alluvium, which contain marine fossils, and which M. Lory believes must be united with the Molasse. On the heights of the Albis near Zurich, on other ridges of Eastern Switzerland, and to the north of the Lake of Constance, there are very thick stratified gravels, partly cemented by calcareous tufa, as to the age of which we are in doubt, some regarding them as tertiaries, others as diluvian. On the other hand, in the environs of Berne and Fribourg, and as far as Lausanne, we see the stratum with erratics resting immediately upon the Molasse; the gravels, equal in thickness to the boulder-formation, fill up the depressions beside it, and extend in many places over the boulder-clay. Here, therefore, the gravel is contemporaneous with, or posterior to the glacial epoch, and appears to be the product of the destruction of the erratic deposits by the action of rivers. Lastly, there are large masses of gravel deposited by rivers which have no lakes in their course, such as the Sarine, the two Emmes, and the Thur. In ancient times they appear to have frequently changed their course. Nevertheless, after deducting all these masses, there still remains the true ancient alluvium, forming the base of the erratic deposit; and of this we must endeavour to explain the transportation. This gravel is well developed on the banks of the Adda and the Dora Riparia, in the environs of Geneva, at the mouth of the Kander in the Lake of Thun, in the neighbourhood of Uznach, and elsewhere. The horizontal strata of this ancient alluvium rest upon the bevelled edges of the inclined strata of Molasse; the date of their formation must therefore necessarily intervene between the catastrophe which elevated the tertiary beds, and the period of the great extension of the glaciers.

M. de Mortillet\* assumes, like M. Desor and the great majority of geologists, that our Alpine lakes are orographic lakes, due to ruptures of the soil which took place when the Alps acquired their present form. He assumes that their basins, before the glacial epoch, were filled with the pebbles and sands which beyond the Alps have also formed the ancient alluvium,

\* *Atti della Soc. Ital.* vol. v. (1863), November.



and that subsequently, these moveable materials being unable to resist the pressure of the glaciers, the basins were cleared out and filled with ice up to the period of the thaw, and of the retreat of the glaciers into the high Alps. M. Desor\* supposes the lakes to have existed until the arrival of the ice, to have been temporarily filled by the glacier which transported the gravel blocks on its surface, and to have returned to the condition of lakes when the glaciers retreated. M. Omboni† combines these two solutions in one. The glaciers, according to him, advanced to the lower extremity of the lakes, replacing the water with ice; torrents issuing from the long-stationary glaciers deposited the ancient alluvium; and, lastly, the glaciers, again advancing, worked up the soil of this alluvium and deposited the moraines and blocks which repose upon it.

None of these geologists doubted the relation between our lakes and the elevation and orography of the Alps. M. Desor explains even his lakes of erosion by the action of powerful currents caused by the retreat of the waters in consequence of the elevation. The theory was carried still further in England. Mr. Ramsay‡, who, from his long-continued researches upon the effects of glacial action in Wales and in the Alps, was prepared to regard the question from a new point of view, sees no essential difference between the small lake-basins of the mountains of Wales, Scotland, and Switzerland, often hollowed out of the solid rock, and the enormous depressions of our great lakes. Attributing the former to the slow erosion of glaciers upon their bottom, he does not hesitate to assume that the same agent has produced the latter, and that all the basins of our great lakes are the result of glacial erosion. As these basins did not exist before the origin of the erratic formation, the transportation of the ancient alluvium by rivers would be explained without any difficulty. This theory of the learned Professor has been favourably received by several geologists of the highest merit. Professor Tyndall§ goes still further. He ascribes not only the basins of our lakes, but the Alpine valleys themselves to the erosion of glaciers. The whole Alpine system, according to this celebrated physicist, formed originally an immense smooth boss or enormous mass, in the surface of which the glaciers, by their gradual advance, hollowed out our valleys. Mr. Beete Jukes||, who had previously indicated the great effects of erosion in the conformation of Ireland, declares himself in favour of this opinion; and

\* *Revue Suisse*, 1860. *Actes de la Soc. de Lugano*, 1861.

† *Atti della Soc. Ital.* vol. v. (1863), November.

‡ *Quart. Journ. Geol. Soc.* vol. xviii. (1862).

§ "Conformation of the Alps," *Phil. Mag.* September 1862.

|| Address to the Geological Section of the British Association, Oct. 1862.

he adds the remark that, according to this theory, the transverse valleys must be more ancient than the longitudinal ones, which explains the bend made by the Rhone at Martigny.

Before seeking for a solution of the problem of the lakes, it appears to be necessary to examine more closely the effects of erosion, which in all the proposed solutions plays an important part. It holds the first place in those of Professors Ramsay and Tyndall, and the second in those of MM. Desor and Mortillet.

No one doubts that erosion has played a great part in the configuration of the valleys and depressions of mountainous countries. When, in our Molassic regions, in Appenzell, in the Emmenthal, and in the neighbourhood of Berne, Fribourg, and Lausanne, we see horizontal or slightly inclined strata cut by large and small valleys ramifying into gorges and ravines which date, so to speak, from the last storm,—when in districts in which the ground is schistose, such as the Simmenthal, certain parts of the Grisons or the Valais, or the woodless mountains of Savoy and Dauphiné, we see each fall of rain give rise to new falls of rock,—lastly, when we consider the great masses of débris which from the most remote periods have been transported beyond the mountains by the glaciers and rivers,—it seems impossible to estimate too highly the influence which the constant action of erosion must have exercised upon the conformation of the country. It is certain that many of our valleys owe to it their origin, and nearly all their present characters. Nevertheless there is a limit which the erosive action of rivers and glaciers does not exceed, and which depends upon the resistance of the bottom, the mass of water or ice, and upon the slope. This limit being attained in favour of the resistance, by the solidity of the bottom or by the diminution of the mass of water or of the slope, the rivers, instead of continuing the erosion, fall in cascades and cataracts, or seek issues which present fewer obstacles, or, if their rapidity permits, they form deposits. From time immemorial the falls of the Rhine, the Toccia, and the Aar, and the cataracts of the Rhine at Laufenburg and those of the Danube and the Nile, have not changed either in place or form. When thus we see a river traverse solid rocks, such as compact limestones, granites, or porphyries, whilst at the same level it might have cut itself a way through softer rocks, we must be convinced that its course has not been impressed upon it by erosion. The Rhine near Sargans had to surmount an elevation of only twenty feet above its highest level in order to throw itself in a straight line into the Lake of Wallenstadt; why then should it have taken its course, making a bend and traversing the calcareous mountains of the Schollberg and Fläscher Berg, if these mountains had not offered it a more ready passage, of a dif-

ferent and more ancient origin? Why should the Simme near Wimmis have forced a passage through the limestones of the Burgfluh, when between the latter and the Niesen there were only schists to be traversed? Why should the Sarine have hollowed out its channel by the long limestone defile from Rossinière to Montbovon, when to the left there was the depression of the Mosses, of which the Flysch rock presented much less resistance? The impossibility of explaining by erosion those ravines which are evidently large crevasses, as also the relation existing between the longitudinal valleys and the strike and dip of the strata, are facts long since established in science. They have set up the conviction that the forces of erosion have not acted alone, but that other very powerful agents have modified and fashioned the surface of the globe. It is half a century since this result was indicated in this same journal by a distinguished physicist\*. In all treatises on geology the origin of each of the different kinds of valleys is deduced from a particular principle.

The reasons which show us the insufficiency of the erosion by rivers to explain the origin of a great number of valleys, acquire much more force in Mr. Ball's excellent memoir if we apply them to glaciers, the effects of which are comparable to those of streams of lava. If the latter possessed the power of working up the soil over which they pass, which is often composed of moveable sand and but slightly coherent tuffs, as it is asserted that the glaciers do, the physiognomy of volcanic cones and districts would be very different from that which we know to belong to them. As in the case of lavas, the retardation of the movements of glaciers upon their bottom is necessarily much greater than with streams of water. Upon the heights, when the mean temperature is below zero (C.), the ice even remains attached to the soil, and the glacier, if it can be formed, only advances at its upper part; but in general the new-fallen snow glides over the solid ice, forming avalanches, and the glacier, remaining unthickened, advances, notwithstanding its often considerable slope, much more slowly than the very large glaciers of the valleys. The latter can hardly exert any very active erosion, their lower surface being often separated from the beds by the water arising from their melting, or by empty spaces, and their movement being far less than that of rivers which appear to us to be stagnant. Nevertheless a certain amount of erosion takes place, as is proved by the turbid water which issues from the glaciers; but its action appears to be limited to rounding off points and salient angles, and polishing and striating the rocks. One of the best ascertained facts is that the erosion of glaciers is distinguished from that of water by the production by the former of

\* *Bibl., Brit.* vol. lix.



convex rocks or *roches moutonnées*, whilst the second gives rise to cavities. The hollows or *marmites* which occur as an exception to this rule in what appears to us to be the rocky bed of ancient glaciers, are justly ascribed to the friction of gravel set in rotation by falls of water through the glacier, or to the *moulins*.

Mr. Ramsay rightly assumes that the basins of our lakes cannot have been hollowed out by running water. A certain incline is necessary to enable rivers, even with a muddy bottom, to hollow out their bed; and this slope does not occur either in the lakes of the Jura or in those of Zurich and Constance. Otherwise, if only a great mass of water were necessary, why do we not find the Nile and the Ganges in their great floods hollowing out basins for themselves? And even if a great inclination be added to the mass of water, and the soil is favourable to erosion, deep holes or *marmites* only are produced, and their extent never exceeds the radius of the direct action of the impact of the water and of the pebbles which it sets in motion. Of this we have the proof in our waterfalls, in those of Italy, and in the cataract of Niagara, so well described by M. Desor, as quoted by M. Mortillet. How, moreover, could we suppose that the Rhine, at so great a distance from the high mountains, has had sufficient force to hollow out a basin like that of the Lake of Constance? On the same principle one might suppose the Caspian Sea to be due to the erosion of the Volga, or the Dead Sea to that of the Jordan, and regard these great depressions as mere effects of the erosion of the rivers which traverse them. And if we assume that the basin of the Lake of Constance extended as far as the Schollberg near Sargans, and that it was subsequently filled up by detritus as far as Rheineck, how can we suppose that the same river, which in the first place hollowed out its bed from the Schollberg to Schaffhausen at more than a thousand feet below the present surface of the soil, should have afterwards filled it up? Those who consider that our lakes have been hollowed out by the action of water maintain no such doctrines. M. Desor, as we have seen, requires great floods caused by the upheaval of the Alps; and the celebrated Escher von der Linth\*, who knew better than anyone the power of rivers, also calls for the intervention of diluvian waters. It is always the great *débâcle* of De Saussure which, in the theories of its learned author and down to our own day, has played so important a part in the geology of Switzerland. But even if, with the view of increasing the force of the shock, we suppose with von Buch† that the waters of the sea were thrown, by the sudden upheaval of the Alps, over the highest

\* Gilbert's *Annalen*, vol. liii. (1816).

† Poggendorff's *Annalen*, vol. ix. (1827).



summits, we may still doubt whether they could have acquired and retained the force necessary to hollow out valleys of the depth of our lakes, and of from 20 to 30 Swiss leagues in length.

However, since the appearance of the classical works of Sir Charles Lyell, these great efforts of the imagination are no longer popular, and the great *débâcle*, which was admitted chiefly in order to explain the transportation of boulders, has given place to the calm and slow movement of the glaciers. It is also by means of the glaciers that De Mortillet, Ramsay, and their adherents suppose the basins of the lakes to have been hollowed out. After what has been said as to the erosive action of glaciers, it is useless to return to this question. I shall confine myself to remarking that upon the bed of gravel and detritus of unknown depth which extends in front of our great glaciers at Chamouni, at the glaciers of Arolla, Ferpècle, and the Aar, and in front of all the others, we do not see the least trace of the asserted tendency of the glaciers to bury themselves by digging out the soil. We also know that in those regions where the glaciers attain the sea-shore, they are prolonged above the water, and do not sink below its level.

Having thus ascertained the insufficiency of erosion for the explanation of the origin of the valleys and lakes of the Alps, we can hardly choose but recognize, with C. Escher, an intimate connexion between a great number of Alpine valleys and the inclined position of the strata in the chains which they separate. These are true orographic valleys, such as M. Desor has pointed out in the Jura; and to the two kinds described by him—the *synclinal* and *isoclinal valleys*—we must add, according to Escher, for the Alps the *anticlinal valleys*, of which the Justithal to the east of the Lake of Thun presents a fine example. As to the *cluses*, with which we shall join those rocky ravines which cut the interior beds of the chains in the direction of the dip, and to which Thurmann has given the name of *Ruz*, these are evidently ruptures, often enlarged by erosion. We shall likewise add the *valleys of sinking* (*vallées d'affaissement*), which indeed do not occur, as far as I know, in the Jura, but play a great part in volcanic countries; and perhaps some flat-bottomed circular valleys in the Alps may be referred to this type.

However, a classification of valleys founded upon the orography of the Jura can only find a very limited application in the Alps. Most of the Alpine valleys, and indeed all those of any extent, are valleys of rupture which cut the strata at more or less oblique angles; frequently also they are complex, and pass from one type to another; and many of them are combined with great faults. Even longitudinal valleys of small size, and apparently quite simple at the first glance, will not conform to

the Jurassic classification. The valley of the Bas-Simmenthal may be classed among the synclinal valleys ; but what a difference there is between it and the valleys of the same kind in the Jura ! At the bottom of the latter the beds are horizontal, and rise to the two sides ; in the Simmenthal they are vertical, and diverge upwards like a fan—it looks like a synclinal valley folded up (*refoulée*) by lateral pressure. The valley of the Pays d'Enhaut, from Gessenay to Château d'Oex, also appears to be longitudinal ; nevertheless it cuts obliquely through three zones of Flysch and two intermediate ranges of limestone which run from N.E. to S.W. The Valais in the neighbourhood of Sion is an isoclinal valley, its two sides inclining to the S.E. ; but its northern side consists of Jurassic limestone, and the opposite side, which ought to be more recent, belongs to the carboniferous series. Here there has evidently been a great fault. The same circumstance occurs again at the Lake of Brienz : the base of the northern range being Neocomian, the base of the group of the Faulhorn, which would be supposed to be superior, is Jurassic ; and the Neocomian strata only occur at the very summit of this group. The relation of the great transverse valleys of the Alps with the *cluses* of the Jura is no better founded : most of these commence by gigantic *ruz*, such as those of the Aar, of the Grimsel at Gutannen, and of the Reuss from the St. Gothard to Amstæg. Further down this character is lost ; the valley, cutting the ranges horizontally, resembles the *cluses*, as that of the Arve from Sallanches to Cluse, that of the Reuss from Amstæg to Brunnen, where it unites with a longitudinal valley, and that of the Aar from Meiringen to Leissigen. But the further we descend, the more we see the difference between its opposite sides increase ; so that a valley which still appears to belong to the class of *cluses*, often separates two systems of mountains of a totally different geological character. This is the case in the valley of the Arve between Bronneville and Annemasse, in that of the Aar upon the margins of the Lake of Thun, in that of the Rhine from Schollberg to Rheineck, and in that of the Iller at Sonthofen. By this circumstance these valleys acquire a significance of much more importance than that of being simple crevasses ; they appear as boundaries, probably of great antiquity, between two different groups of mountains, such as would be a boundary placed between the Alps and the Jura, or between the latter and the Vosges. And, lastly, how are we to class the great semicircular valley which extends from Reichenau by Coire to the Lake of Wallenstadt, resembling, on a colossal scale, the Atrio dei Cavalli of Vesuvius ? Moreover, this does not appear to be unique in the Alps. We may find a parallel to it in the valley which follows the Rhone from Mar-

tigny to Geneva, and in that which is prolonged from the Lake of Annecy to Faverges. As the latter circle appears to be dependent on Mont Blanc, so that of Eastern Switzerland bounds the spreading foot of the Verrucano of Glaris.

Those valleys which have cracked the Alpine strata are generally regarded as traces of the upheavals, dislocations, and twistings which that country has undergone; and one of the most powerful of these revolutions must be more recent than the Miocene period, as the strata of the Nagelflue and Molasse have been so much affected by it as to have acquired greatly inclined and even vertical positions. On the other hand, we cannot overlook a certain connexion between the basins of the lakes and the valleys in which they are situated. These basins appear as the remains of the original depth of the valleys, before their bottoms were partially filled up by rivers. It has always been admitted that originally the basin of the Lake of Geneva extended on one side as far as St. Maurice, and on the other beyond Carouge; it has also been supposed that the two basins of the lakes of Brienz and Thun only formed a single one, which extended upwards as far as Meiringen, and downwards to the vicinity of the Belpberg, and perhaps further; and, lastly, that the three lakes of the Jura were not separated as at present by marshes, and that the great basin which contained them must have extended from Enteroches and Payerne as far as below Soleure.

The connexion between the basins of the lakes and the tilting of the calcareous and Miocene beds of the Alpine and Jurassic ranges being admitted, we find ourselves face to face with the difficulty already indicated, namely, the transportation of the ancient alluvium beyond the Alps across the lakes; and after having got rid of the proposed solutions, we have only, it seems to me, two ways to escape from this difficulty—two solutions, moreover, which do not mutually exclude each other, and which may be applied according to the configuration presented by the different localities.

We may suppose the deposits of the ancient alluvium to have been produced by rivers which have no lakes to traverse; and these rivers having frequently changed their course, this explanation may be adapted to cases which at the first glance seem to be opposed to it. The beds of sand and gravel which, at the mouth of the Kander in the Lake of Thun, support the moraine of Strätligen, are undoubtedly an ancient deposit of the Kander and the Simme; the great plain between Thun and Thierachern must have had the same origin. All the pebbles on the shore of the lake, as far as the Schadau, are derived from the valleys to the westward of the lake; the ridges of Nagelflue east of the Aar have not furnished a single one. In the same way the



position of a great part of the ancient gravels below Geneva is explained by the deposits of the Arve, and a portion of those of the Lake of Zurich by the deposits of the Sihl and the Linth.

I do not, however, conceal from myself that this solution cannot be generally admitted. It does not apply to the ancient alluvia to the south of the lakes of the Italian slope of the Alps; it does not explain the lignitic formation of Uznach, the level of which is considerably above that of the Linth; nor the stratified gravels, older than the glacial deposit, to the north of the Lake of Constance. Even for the Lake of Thun the explanation given does not suffice, inasmuch as we find between Uttigen and Kiesen, two leagues below Thun, vast accumulations of ancient gravel rising to more than 100 feet above the level of the Aar. In these cases we must necessarily have recourse to depressions which have taken place after the deposition of the gravel. M. Guyot\* prefers this solution to any other for explaining the origin of the lakes of the Jura, without being driven to it by deposits of ancient alluvium, which do not occur on the left bank of the lakes. In the ridge of Molasse known as the *Molle*, which rises from the bottom of the Lake of Neuchâtel to within 30 feet of the surface of the water, as also in that which, in the Lake of Bienné, unites the Jolimont to the island of St. Pierre, he sees submerged ridges of the same order as those of the valley and the Jolimont still occupying their original position. These sublacustrine ridges, moreover, cannot well accord with the supposition that the lakes had their origin by erosion, whether by means of currents of water or of glaciers, the molasse not being sufficiently solid to resist this.

If we admit the formation of the basins of our lakes by depressions, the deposits of ancient alluvium beyond these basins compel us to assume that the bottom of the great crevasses which form our valleys has been entirely filled up, as is still the case with that of the Arve, that of the Sarine, and others. Although there can have been no deficiency of débris after one of the most terrible convulsions, this filling up undoubtedly requires that an exceedingly long time must have elapsed between the formation of the crevasses and that of the ancient alluvium. Fortunately another consideration, which has nothing hypothetical about it, leads us to the same conclusion, namely, the great difference between the fauna and flora of the last sediments of the Molasse and the first of the ancient alluvium. It requires time, and indeed a long time, to allow a flora analogous to that of the Southern States of North America to give place to that which now grows in our country; and we find the latter represented even in the diluvial lignites. It is probable also that much time would be necessary

\* *Mém. de Neuchâtel*, vol. iii.



for the *Anthracotheria*, Tapirs, and Mastodons of the Molasse to yield their place to Elephants, and to Ruminants which appear to be the source of our existing Bovine races. The idea that depressions took place along the lines of the crevasses of the valleys after the latter had been covered by a new soil need not astonish us. Great spaces at the bottom of the fractures may have remained unfilled; narrowings of the rock, or the large size of the first blocks engulfed, may have arrested the filling up of the valley; but subsequently an increase in the weight of this temporary roof, or perhaps the addition of that of the great glaciers of the diluvial period, may have caused the obstacle to yield, and the soil which it supported to fall in.

LXXIV. *Note on the Theory of Cubic Surfaces.*

By A. CAYLEY, Esq.\*

THE equation

$$AX^3 + BY^3 + 6CRST = 0,$$

where  $X + Y + R + S + T = 0$ , represents a cubic surface of a special form, viz. each of the planes  $R=0$ ,  $S=0$ ,  $T=0$  is a triple tangent plane meeting the surface in three lines *which pass through a point*†; and, moreover, the three planes  $AX^3 + BY^3 = 0$  are triple tangent planes intersecting in a line. It is worth noticing that the equation of the surface may also be written

$$ax^3 + by^3 + c(u^3 + v^3 + w^3) = 0,$$

where  $x + y + u + v + w = 0$ . In fact, the coordinates satisfying the foregoing linear equations respectively, we have to show that the equation

$$AX^3 + BY^3 + 6CRST = ax^3 + by^3 + c(u^3 + v^3 + w^3)$$

\* Communicated by the Author.

† The tangent plane of a surface intersects the surface in a curve having at the point of contact a double point, and in like manner a triple tangent plane intersects the surface in a curve with three double points, viz. each point of contact is a double point; there is not in general any triple tangent plane such that the three points of contact come together; or, what is the same thing, there is not in general any tangent plane intersecting the surface in a curve having at the point of contact a triple point. A surface may, however, have the kind of singularity just referred to, viz. a tangent plane intersecting the surface in a curve having at the point of contact a triple point; such tangent plane may be termed a 'tritom' tangent plane, and its point of contact a 'tritom' point: for a cubic surface the intersection by a tritom tangent plane is of course a system of three lines meeting in the tritom point. The tritom singularity is sibi-reciprocal; it is, I think, a singularity which should be considered in the theory of reciprocal surfaces.

may be identically satisfied. We have

$$\begin{aligned} & ax^3 + by^3 + c(u^3 + v^3 + w^3) \\ &= ax^3 + by^3 + c[(u + v + w)^3 - 3(v + w)(w + u)(u + v)] \\ &= ax^3 + by^3 - c(x + y)^3 - 3c(v + w)(w + u)(u + v), \end{aligned}$$

which is to be

$$= AX^3 + BY^3 + 6CRST;$$

and we may find X, Y, R, S, T, linear functions of  $x, y, u, v, w$ , so as to satisfy these equations, and so that in virtue of

$$x + y + u + v + w = 0,$$

we shall have also  $X + Y + R + S + T = 0$ . For, assuming

$$AX^3 + BY^3 = ax^3 + by^3 - c(x + y)^3,$$

$$X + Y = x + y,$$

$$R = \frac{1}{2}(v + w), \quad C = -4c,$$

$$S = \frac{1}{2}(w + u),$$

$$T = \frac{1}{2}(u + v),$$

we have identically

$$AX^3 + BY^3 + 6CRST = ax^3 + by^3 - c(x + y)^3 - 3c(v + w)(w + u)(u + v),$$

$$X + Y + R + S + T = x + y + u + v + w;$$

and thus it only remains to show that we can find X, Y linear functions of  $x, y$ , such that

$$AX^3 + BY^3 = ax^3 + by^3 - c(x + y)^3,$$

$$X + Y = x + y.$$

This is always possible; in fact if

$$U = ax^3 + by^3 - c(x + y)^3,$$

then taking  $\Phi$  for the cubicovariant, and  $\square$  for the discriminant of U, we have  $\frac{1}{2}(\Phi + \sqrt{\square}U)$ ,  $\frac{1}{2}(\Phi - \sqrt{\square}U)$  each a perfect cube, say

$$\frac{1}{2}(\Phi + \sqrt{\square}U) = (\lambda x + \mu y)^3,$$

$$\frac{1}{2}(\Phi - \sqrt{\square}U) = (\nu x + \rho y)^3,$$

and we then have

$$U = \frac{1}{\sqrt{\square}} \{ \lambda x + \mu y \}^3 - (vx + \rho y)^3 = AX^3 + BY^3,$$

which is satisfied by

$$X = l(\lambda x + \mu y),$$

$$Y = m(vx + \rho y),$$

if

$$Al^3 = \frac{1}{\sqrt{\square}}, \quad Bm^3 = -\frac{1}{\sqrt{\square}}.$$

The equation  $X + Y = x + y$  then gives

$$l\lambda + m\nu = 1,$$

$$l\mu + m\rho = 1,$$

which give the values of  $l$  and  $m$ , and thence the values of  $A$  and  $B$ ; and collecting all the equations, we have

$$X = \frac{\rho - \nu}{\lambda\rho - \mu\nu} (\lambda x + \mu y), \quad A = \frac{1}{\sqrt{\square}} \left( \frac{\lambda\rho - \mu\nu}{\rho - \nu} \right)^3,$$

$$Y = -\frac{\mu - \lambda}{\lambda\rho - \mu\nu} (vx + \rho y), \quad B = \frac{1}{\sqrt{\square}} \left( \frac{\lambda\rho - \mu\nu}{\rho - \nu} \right)^3,$$

$$R = \frac{1}{2}(v + w), \quad C = -4c,$$

$$S = \frac{1}{2}(w + u),$$

$$T = \frac{1}{2}(u + v),$$

where

$$\lambda x + \mu y = \left\{ \frac{1}{2}(\Phi + \sqrt{\square} U) \right\}^{\frac{1}{3}},$$

$$\mu x + \rho y = \left\{ \frac{1}{2}(\Phi - \sqrt{\square} U) \right\}^{\frac{1}{3}}$$

( $\Phi$ ,  $\square$  being respectively the cubicovariant and the discriminant of  $U = ax^3 + by^3 - c(x + y)^3$ ), for the formulæ of the transformation

$$AX^3 + BY^3 + 6CRST = ax^3 + by^3 + c(u^3 + v^3 + w^3),$$

$$X + Y + R + S + T = x + y + u + v + w.$$

The equation  $ax^3 + by^3 + c(u^3 + v^3 + w^3) = 0$ , where

$$x + y + u + v + w = 0,$$

presents over the other form the advantage that it is included as a particular case under the equation  $ax^3 + by^3 + cu^3 + dv^3 + ew^3 = 0$  (where  $x + y + u + v + w = 0$ ) employed by Dr. Salmon as the canonical form of equation for the general cubic surface.

5 Downing Terrace, Cambridge,  
April 29, 1864.

LXXV. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 115.]

THE following are the results at which Persoz has arrived after an investigation of tungsten\*.

From the constitution and properties of its oxygen compounds, tungsten belongs to the group of biatomic elements arsenic, antimony, and phosphorus.

Its equivalent ( $O = 8$ ), as deduced from numerous experiments, is  $153 \cdot 3 = W$ .

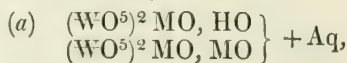
It forms two oxidized compounds; tungstic oxide,  $WO^3$ , and tungstic acid,  $WO^5$ .

By their combination these two compounds can form a third oxide (of the nature of Berzelius and Dumas's *saline oxides*), which corresponds to the formula  $WO^3 + WO^5 = 2WO^4$ .

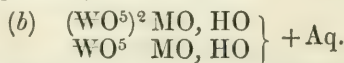
Tungstic acid is polybasic; its simple and double salts are represented by the general formulæ  $WO^5$ ,  $MO$ ,  $HO + nAq$ , and  $WO^5$ ,  $MO$ ,  $MO + nAq$ .

Like its congeners, phosphoric and antimonie acids, tungstic acid may become artificially modified by heat, so that its capacity of saturation is reduced to one-half; it may be said then to give rise to a new acid, *metatungstic acid*, the existence of which depends on definite conditions. Its formula is  $(WO^5)^2$  or  $W^2O^{10}$ .

The simple metatungstates are represented by the formula  $(WO^3)^2MO$ ,  $HO + Aq$ . They readily form double salts by combining either with each other,



or with the simple tungstates,



The paratungstates and certain acid tungstates come under these formulæ.

Sulphur, chlorine, and bromine combine with tungsten to

\* *Comptes Rendus*, November 9, 1863.



produce compounds which correspond exactly to oxides and acids.

Tungsten does not form an oxychloride. The compounds which have been thus designated are combinations in definite but variable proportions of *anhydrous acid* with the corresponding *chloride*.

Böttger describes\* further experiments which he has instituted for the purpose of finding simpler methods of preparing thallium. The deposit which he investigated was from a sulphuric acid-works near Aachen, in which Belgian pyrites are used. The method which he uses, and which he recommends in preference to that given before†, is based on the discovery he has made of a higher sulphur compound than that previously described, and which is characterized by its unalterability in the air and in water, and by its insolubility in acids.

The finely powdered deposit is boiled out with four to six times its weight of distilled water, and then a concentrated solution of hyposulphite of soda is added until a turbidity, or rather a yellowish coloration ensues, which, on further addition of hyposulphite, changes into a copious flocculent deposit of a litharge colour. When this colour begins to change into a yellow, the addition of hyposulphite must be stopped. The precipitate consists of a new sulphide of thallium, of sulphide of arsenic, and some free sulphur. It is well washed out, and at first by decantation, by which much of the sulphur is removed; it is then boiled with caustic soda or cyanide of potassium until the precipitate is of a pure greyish-black colour, that is, until all free sulphur and sulphide of arsenic are removed and the lower sulphide of thallium is formed. This is then washed, dissolved in sulphuric acid, and the thallium precipitated in the usual manner by zinc.

Another method of extracting thallium from the residue consists in treating the aqueous decoction with sulphurous acid, and then adding iodide of potassium as long as a precipitate of iodide of thallium forms. When this is diffused in water containing some potash, and sulphuretted hydrogen passed through, the whole of the thallium is obtained as the sulphide.

Hübner and Wehrhane‡ have prepared a compound of phosphorus with cyanogen. Perfectly dry cyanide of silver was mixed with a corresponding quantity of terchloride of phosphorus diluted with chloroform, and the mixture enclosed in

\* Liebig's *Annalen*, November 1863.

† *Phil. Mag.* vol. xxvi. p. 536.

‡ Liebig's *Annalen*, November 1863.

a sealed tube and heated from  $120^{\circ}$  to  $140^{\circ}$  for several hours. The tube was then opened, the chloroform evaporated, and the contents of the tube brought into a small stoppered retort, which was then placed in an oil-bath and the cyanide of phosphorus driven into the neck of the retort by being heated to  $160^{\circ}$  to  $190^{\circ}$  in a current of carbonic acid. The cyanide of phosphorus forms then brilliant white needles an inch in length, or else hexagonal plates. When these crystals are slightly warmed, they inflame in the air and burn with a bright light. They decompose with water, alkalies, or acids, with the formation of hydrocyanic and phosphorous acids. The crystals melt and volatilize at  $190^{\circ}$ : their composition, as determined by analysis, is  $P(CN)^3$ ; further experiments are required to ascertain if there is a compound  $P(CN)^5$ .

Hahn has investigated\* the nature of the products which are formed when cast iron is dissolved, more especially with a view to ascertaining the compounds and forms into which the carbon passes in this case. He has also investigated some of the compounds of silicon with iron.

Schafhäütl had supposed that among the gases disengaged when iron was dissolved, olefiant gas and hydride of ethylene might be present. Hahn collected some of the gas liberated when grey iron was dissolved in  $HCl$ , and, after it had been washed with solutions of potash and of copper to remove  $SH$  and  $PH^3$ , it was treated in a long tube with a coke bullet soaked with sulphuric acid. A slight diminution in volume was observed, which indicated the presence of a gas of the formula  $C^n H^{2n}$ . In order to ascertain which of these it was, the gas disengaged from white iron, after being washed by water,  $KO$ , and  $CuO$ , was passed into bromine, which was thus converted into a heavy oily liquid, which was washed with potash and dried over  $CaCl$ . On subsequent fractional distillation and analysis of the various fractions, it was found to be a mixture of the bromides of ethylene,  $C^2 H^4 Br^2$ , of propylene,  $C^3 H^6 Br^2$ , of butylene,  $C^4 H^8 Br^2$ , of amylene,  $C^5 H^{10} Br^2$ , and of caproylene,  $C^6 H^{12} Br^2$ . A direct experiment was made to see if the gas contained any acetylene, by passing it into ammoniacal solution of copper; but no explosive compound of cupride of acetylene was obtained.

The presence of the above compounds is explained by the fact that, as white iron is difficultly soluble in cold dilute acid, the flask in which it was contained was heated; hence the gases at a temperature of  $100^{\circ}$  had carried several of the liquid hydrocarbons, amylene, caproylene, and probably  $\alpha$ -naphthylene, through the wash bottles to the bromine. In the retort, after rectifica-

\* Liebig's *Annalen*, January 1864.

tion, a tolerable quantity of carbon was left, which speaks for the presence of the latter body.

The nature of the oil produced when iron is dissolved has been frequently the subject of inquiry. Hahn found that the quantity of oil from grey iron was very small; 400 grammes only gave a few drops; but the quantity from white iron was much more abundant.

A portion of this oil gave on analysis numbers which agree with the formula  $C^n H^{2n}$ . It is colourless, limpid, lighter than water, somewhat thicker at  $-20^\circ$ . Its odour is very intense and unpleasant, and it is to it that the peculiar smell of the gases from iron is mainly due. It is soluble in sulphuric acid, and is deposited again on dilution with water.

A quantity of this oil was dehydrated and distilled. It began to distil at  $110^\circ$ , and the temperature rose to  $290^\circ$ , by which time everything had passed over. Vapour-density determinations of the various products showed that the oil is probably a mixture of bodies belonging to the ethylene series, and includes the following members:—

Enanthylene,	$C^7 H^{14}$ ,	boils at	$95^\circ$
Caproylene,	$C^8 H^{16}$	„	125
Nonylene,	$C^9 H^{18}$	„	144 (?)
Paramylene,	$C^{10} H^{20}$	„	163 (?)
Cetene,	$C^{16} H^{32}$	„	125

Besides the above, there are other carbonaceous substances which remain when iron is dissolved; these are mainly resinous products resulting from the oxidation of the etherial oils.

Hahn made some attempts to obtain a siliciuret of iron. When protochloride of iron and chloride of sodium (obtained by melting 40 grms. reduced iron, 150 grms. sal-ammoniac, and 80 grms. chloride of sodium until the excess of sal-ammoniac was expelled), mixed with 5 grms. silicium and 25 grms. sodium, were fused with fluor-spar, a well-melted regulus was obtained about 25 grms. in weight. It was quite homogeneous, white, and the fracture had large reflecting surfaces. It was very brittle; could only be dissolved in HCl in very fine powder, and the gas disengaged contained siliciuretted hydrogen; but by dilute HFl it was dissolved in the cold and in pieces. It was feebly magnetic. Its composition was  $Fe^2 Si$ .

The following experiment by Schiel\* appears to establish the existence of a bromocarbonic acid. A glass bulb with a drawn-out point was filled with bromine and placed in a drawn-out glass flask provided with a cap, by which it could be connected on the one

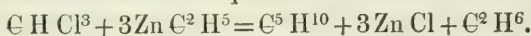
\* Liebig's *Annalen* (Supplement), vol. xi. part 3.



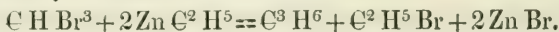
hand with an air-pump, and on the other hand with a gas-holder of carbonic oxide. It was then several times alternately exhausted and carbonic oxide allowed to enter. When the flask was full of pure and dry gas, it was hermetically sealed and the bulb broken. The bromine vapour made the balloon almost opaque; its quantity was only so much that about one-half of the carbonic oxide could be changed into  $\text{C}\Theta\text{Br}^2$ . The flask was then exposed to the sunlight, by which it became somewhat decolorized, but the decolorization was not complete even after standing a couple of years. The point of the flask was opened under caustic potash; a rapid decomposition of the bromocarbonic acid took place. The products of this decomposition (carbonic and hydrobromic acids) were absorbed by the potash, and the residue, amounting to one-half of the total, consisted of pure carbonic oxide. Of course, if the flask had contained nothing but its original charge of carbonic oxide and bromine vapour, this could not have taken place.

Messrs. Alexeyelf and Beilstein\* have still further simplified the method of preparing zincethyle, which the latter chemist devised in conjunction with Rieth†. Instead of the alloy of zinc and sodium, they now use ordinary zinc turnings previously dried over sulphuric acid, but not otherwise specially prepared, and a small quantity of the alloy. For 100 grms. of iodide of ethyle, they take 7 to 8 of alloy and 70 to 80 of zinc; the operation appears to proceed much more easily, and the yield is always in accordance with the theory.

On zincethyle these chemists have tried the action of bromoform; that of chloroform they had already tried, and found it to be in accordance with the equation



But the action of bromoform frequently differs from that of chloroform, for instance in the case of sodium-alcohol. When bromoform was dropped into zincethyle the action was very brisk. The products disengaged were passed through an artificially cooled tube, and then into a bulb apparatus containing bromine. In the cooled tube some bromide of ethyle was condensed, while the other gases disengaged were completely absorbed by bromine; and on treating the bromine with caustic soda, an oil was left which was found to be *bromide of propylene*. Hence the action was probably as follows:—



Bromoform.

Propylene. Bromide  
of ethyle.

\* *Comptes Rendus*, January 18, 1864.

† *Phil. Mag.* vol. xxiv. p. 306.



The bromide of propylene was treated with ethylate of soda, and the disengaged gas led into an ammoniacal solution of protochloride of copper. The yellow deposit characteristic of *allylide of copper* was obtained; hence it may be doubted whether the propylene formed synthetically by the addition of the two radicals  $\text{CH}$  and  $\text{C}^2\text{H}^5$  is identical with ordinary propylene.

The bromides of the hydrocarbons of the general formula  $\text{C}^n\text{H}^{2n}\text{Br}^2$  can lose a molecule of hydrobromic acid and form a brominated derivative of the primitive hydrocarbon, and then a second molecule in becoming changed into a hydrocarbon  $\text{C}^n\text{H}^{2n-2}$  which differs from the original by two of hydrogen. By this method the following hydrocarbons have been obtained:

Acetylene,	$\text{C}^2\text{H}^2$	Berthelot*.
Allylene,	$\text{C}^3\text{H}^4$	Sawitzsch†.
Crotonylene,	$\text{C}^4\text{H}^6$	Caventou‡.

To this series Reboul§ has added, by the same method, the fourth member, *valerylene*,  $\text{C}^5\text{H}^8$ . He obtained it by heating, for several hours in closed tubes at  $140^\circ$ , brominated amylene with a saturated alcoholic solution of potash. Water added to the product of the reaction separated a mixture of valerylene, alcohol, and brominated amylene. This was washed with cold water to remove the alcohol, and the valerylene separated from the brominated amylene by rectification, which was very easy, as the boiling-point of the former is at  $44^\circ$  to  $46^\circ$ ; that of the latter at  $114^\circ$  to  $116^\circ\text{C}$ .

Valerylene is a colourless mobile liquid, much lighter than water, and with a penetrating alliaceous odour. It does not affect an ammoniacal solution of protochloride of copper. It combines with bromine with great energy, forming a bibromide,  $\text{C}^5\text{H}^8\text{Br}^2$ , which is not, however, volatile without decomposition. When treated with potash, this compound is decomposed into bromide of potassium and a liquid with an alliaceous odour, which is probably brominated valerylene,  $\text{C}^5\text{H}^7\text{Br}$ .

Strecker has described a new class of nitrogen compounds||. The action of nascent hydrogen has been tried on nitro-compounds, such as nitrobenzole,  $\text{C}^{12}\text{H}^5(\text{NO}^4)$ , and nitrobenzoic acid,  $\text{C}^{14}\text{H}^5(\text{NO}^4)\text{O}^4$ , has been tried; and the general result is that 4 eqivs. of oxygen are withdrawn and their place taken by 2 eqivs. of hydrogen: it may be assumed that  $\text{NO}^4$  is changed

\* Phil. Mag. S. 4. vol. xxi. p. 358.

† Ibid. p. 359.

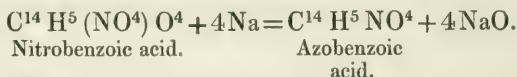
‡ Ibid. vol. xxv. p. 543.

§ Comptes Rendus, January 25, 1864.

|| Ibid. February 1864.

into  $\text{NH}^2$  (amide). Strecker has now found that by the action of sodium on nitro-compounds, oxygen is simply removed, and thus a class of entirely new compounds is formed. The first experiments were made with nitrobenzoic acid, which was prepared by Gerland's method.

When a concentrated solution of this acid was treated with sodium-amalgam, no trace of hydrogen was given off, and the solution became so much heated that it had to be artificially cooled. The liquid became transiently yellow, and finally assumed an orange-yellow colour; no trace of ammonia was liberated. When dilute acid was added to the boiling solution to which alcohol had been added, a pulverulent precipitate was obtained, which could be readily collected on a filter and washed. This is a new organic acid; Strecker names it *Azobenzoic acid*. It is very difficultly soluble in water, alcohol, and ether; so that it cannot be recrystallized from these media. It dissolves in sulphuric acid, and is reprecipitated on the addition of water. It loses nothing in weight, either at  $100^\circ$  or at  $170^\circ \text{C.}$ , and at a higher temperature melts, emitting a yellow vapour. Analysis gave for it the formula  $\text{C}^{28} \text{H}^{11} \text{N}^2 \text{O}^9$ ; and it is probable that it contains an atom of water, which is of the nature of water of crystallization; its formula would thus be  $\text{C}^{28} \text{H}^{10} \text{N}^2 \text{O}^8 + \text{HO}$ : for the dry acid the simplest expression is  $\text{C}^{14} \text{H}^5 \text{NO}^4$ ; and its formation would then be thus expressed,



It dissolves in ammonia, caustic and carbonated alkalies. Its ammoniacal solution loses on evaporation in water part of its ammonia, and leaves an acid ammonia-salt.

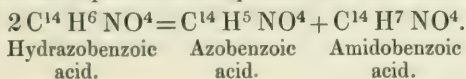
When a solution of azobenzoate of ammonia is added to chloride of barium at the boiling temperature, an orange-yellow granular precipitate, consisting of microscopic rhombic laminæ, is deposited, which is almost insoluble in alcohol. The air-dried salt has the formula  $\text{BaO}, \text{C}^{14} \text{H}^4 \text{NO}^3 + 5\text{HO}$ , of which it loses 4 equivs. of water at  $100^\circ$ , and apparently another equivalent at  $140^\circ$ .

*Azobenzoate of lime* is obtained in the same way as the baryta-salt. *Azobenzoate of silver* is a pulverulent yellow body, which contains no water, and has the formula  $\text{C}^{14} \text{H}^4 \text{Ag NO}^4$ . *Azobenzoic ether* ( $\text{C}^{18} \text{H}^9 \text{NO}^4 = \text{C}^4 \text{H}^5 \text{O}, \text{C}^{14} \text{H}^4 \text{NO}^3$ ) is best obtained by adding sodium-amalgam to nitrobenzoic ether slightly acidified by acetic acid. Crystallized from alcohol, it forms splendid long lustrous needles. When heated in a close tube with ammonia, the amide of azobenzoic acid appeared to be formed;

and this body also seemed to result from the action of sodium-amalgam on nitrobenzamide.

When a solution of azobenzoate of soda in excess of caustic soda was boiled with solution of green vitriol, hydrated oxide of iron was formed at first, which speedily changed into black protosesquioxide of iron on the addition of more solution of protoxide. The solution was feebly yellow, and on the addition of an acid a yellowish white precipitate was formed, which Strecker calls *hydrazobenzoic acid*. It is nearly insoluble in boiling water, and dissolves in alcohol with difficulty, but sufficiently so for recrystallization, by which it is obtained in yellowish white indistinct flakes. It has the formula  $C^{14}H^6NO^4$ , differing from azobenzoic acid by containing an additional atom of hydrogen.

The new acid readily dissolves in the alkalies with a clear yellow colour, but on standing in the air these solutions rapidly absorb oxygen and are converted into the azobenzoates. When boiled with strong hydrochloric acid, hydrazobenzoic acid is decomposed into azobenzoic acid and hydrochlorate of amidobenzoic acid. The decomposition is expressed as follows:—



Schwanert\* has published the results of a lengthened investigation on the action of nitric acid on camphor, etherial oils, and resins. In the case of camphor, about half a pound was heated with ten to twelve times its weight of nitric acid, with frequent cohobation, for about twenty hours. By this time the camphor had completely disappeared, and the disengagement of nitrous fumes had almost entirely ceased. When the liquid was evaporated to about one half, camphoric acid crystallized out, and on further evaporation it yielded another crop of crystals, while a mass was left in all respects resembling venetian turpentine. Besides these products, water and carbonic acid were also formed.

The above mass Schwanert found to consist essentially of a new tribasic acid, the formula of which is  $C^{10}H^{14}O^7$ , and which is formed not only from camphor, but also from etherial oils and resins. Schwanert names it *camphresic acid*. It is purified by solution in water, neutralization with ammonia, and partial precipitation with acetate of lead, by which the whole of the camphoric acid is first precipitated; this is filtered off, the precipitation completed by acetate of lead, the precipitate decomposed by sulphuretted hydrogen; the liquid, filtered off from the sulphide of lead and evaporated, leaves camphresic acid in a state of purity.

\* Liebig's *Annalen*, October 1863.



Camphresic acid has the appearance above described only when it is at once completely evaporated. If its aqueous solution is allowed to stand *in vacuo* over sulphuric acid, it is gradually deposited as a solid, white, granular mass.

When camphresic acid is subjected to destructive distillation, an acid aromatic distillate passes over which contains acetic acid and acetone; at about 200° C. an oily distillate condenses in the retort-neck, while carbonic acid is disengaged, and after a short time tabular crystals are deposited. If then the temperature is raised to 270°, a white crystalline sublimate condenses in the neck of the retort, and some quantity of porous charcoal is left. The quantity of this oil is greater the slower the heat is applied, and the more carefully it is kept between 180° and 200° C.

When the mother-liquor is poured off from the crystals and distilled, it passes over completely between 200° and 210°. It is a clear pale yellow thickish liquid, with an aromatic odour, heavier than water, but not soluble in it; it dissolves, however, in alcohol, and has then a powerful acid reaction. It is a tribasic acid, of the formula  $\text{C}^{10} \text{H}^{14} \text{O}^4$ ; considering the mode of its formation, Schwanert names it *pyrocamphresic acid*. It forms amorphous salts with barium and lead.

The tabular crystals which are formed in crude pyrocamphresic acid dissolve readily in alcohol, and may be purified by treating their alcoholic solution with animal charcoal. They then form transparent rhombic plates which melt at 89° C.; they dissolve in hot water with a rotatory motion. This body is also a tribasic acid, but contains 2 atoms less of  $\text{H}^2 \text{O}$  than camphresic acid, and has thus the formula  $\text{C}^{10} \text{H}^{10} \text{O}^5$ ; Schwanert names it *metacamphresic acid*.

The white crystalline sublimate which forms in the neck of the retort when camphresic acid is heated from 220°–270° C., is in all its properties identical with anhydrous camphoric acid.

The action of *pentachloride of phosphorus*, *concentrated sulphuric acid*, and *sulphuric anhydride* on camphresic acid gave no definite results. Bromine decomposes it, forming carbonic acid, a brominated acid, and bromoform. A body resulting from the action of hydriodic acid on it is still under investigation.

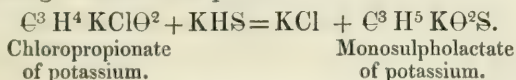
None of the salts of camphresic acid crystallize. When camphresate of calcium is subjected to destructive distillation, it yields *phorone*.

*Camphresic ether*,  $\text{C}^{10} \text{H}^{11} (\text{C}^2 \text{H}^5)^3 \text{O}^7$ , was obtained by the action of iodide of ethyle on camphresate of silver. *Biethylcamphresic acid*,  $\text{C}^{10} \text{H}^{12} (\text{C}^2 \text{H}^5)^2 \text{O}^7$ , was obtained along with some of the ether, when a solution of camphresic acid in absolute alcohol was saturated with hydrochloric acid gas. The acid was removed from



the ether by treatment with weak caustic soda. *Ethylcamphresic acid*,  $\text{C}^{10}\text{H}^{13}(\text{C}^2\text{H}^5)\text{O}^7$ , was obtained mixed with the preceding compound when camphresic acid was boiled with absolute alcohol. Schwanert tried the action of boiling nitric acid on a great number of oils, resins, and gum-resins, and found that camphresic acid was a frequent product of this action; for the details of the experiments the paper must be consulted.

When chloracetic acid is treated with potash, or when chloracetate of potash is boiled with water, chloride of potassium is formed together with glycolic acid. This reaction, which was first studied by Kekulé\*, has been applied in other cases as a means of passing from the acetic acid to the lactic acid series. Carius has further shown† that when chloracetate of potash is treated with hydrosulphate of potassium, the monosulphoglycolic acid is formed; more recently‡ Schacht has applied the same reaction to the preparation of monosulpholactic acid. He first prepared chlorolactate of potash, and treated this at a temperature slightly over  $100^\circ$  with hydrosulphate of potassium, when the following reaction took place:—



The solution was saturated with sulphuretted hydrogen, diluted, and precipitated with acetate of lead; the precipitate was diffused in water, then decomposed by sulphuretted hydrogen, and the liquid, after removing hydrochloric acid, treated with carbonate of barium, by which the barium-salt was obtained. From the barium-salt the lead-salt was prepared, from which in turn the free acid was produced by treatment with sulphuretted hydrogen.

Pure monosulpholactic acid crystallizes in broad needles which are grouped in masses. It is soluble in the ordinary media, and its dilute solution can be boiled without its being decomposed. The acid forms a crystalline but very deliquescent potash-salt, and with silver a white amorphous precipitate.

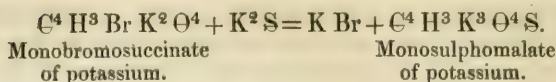
When the acid is treated with dilute nitric acid, it yields an acid identical with one obtained by Buckton and Hofmann in the action of strong sulphuric acid on propionamide.

Carius has prepared monosulphomalic acid by the action of monobromosuccinic acid on sulphide of potassium. The reaction required a temperature of above  $110^\circ$ . It is expressed by the following equation:—

\* Phil. Mag. vol. xvi. p. 138.

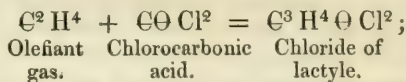
† Liebig's *Annalen*, vol. cxxiv. p. 43.

‡ Ibid. January 1864.



Monosulphomalic acid, as prepared from the liquid resulting from the above reaction by a process essentially the same as that for the preparation of monosulpholactic acid, is an indistinctly crystalline, deliquescent, strongly acid substance, which becomes brown at 100°, and decomposes when heated more strongly. Its barium-, lead-, and silver-salts are all amorphous precipitates. When oxidized with nitric acid, it yields a sulphoacid, the sulphosuccinic acid.

Lippmann\* has found that chlorocarbonic acid unites directly with olefant gas. Dry ethylene gas was passed into a flask containing chlorocarbonic acid. Combination took place, and a product condensed on the sides of the flask in oily drops, which could not, however, be obtained in large quantities, owing to the difficulties of the experiment. The product was treated with water, in which it at once dissolved with formation of hydrochloric acid and a new organic acid. This acid was found on investigation to be monochloropropionic acid,  $\text{C}^3 \text{H}^5 \text{ClO}^2$ . Hence olefant gas and chlorocarbonic acid had simply combined to form chloride of lactyle; thus



and the chloride of lactyle by treatment with water was converted into hydrochloric and chloropropionic acids. From chloropropionic acid lactic acid was obtained by heating the barium-salt of the former acid with water, in which chloride of barium and lactic acid were formed.

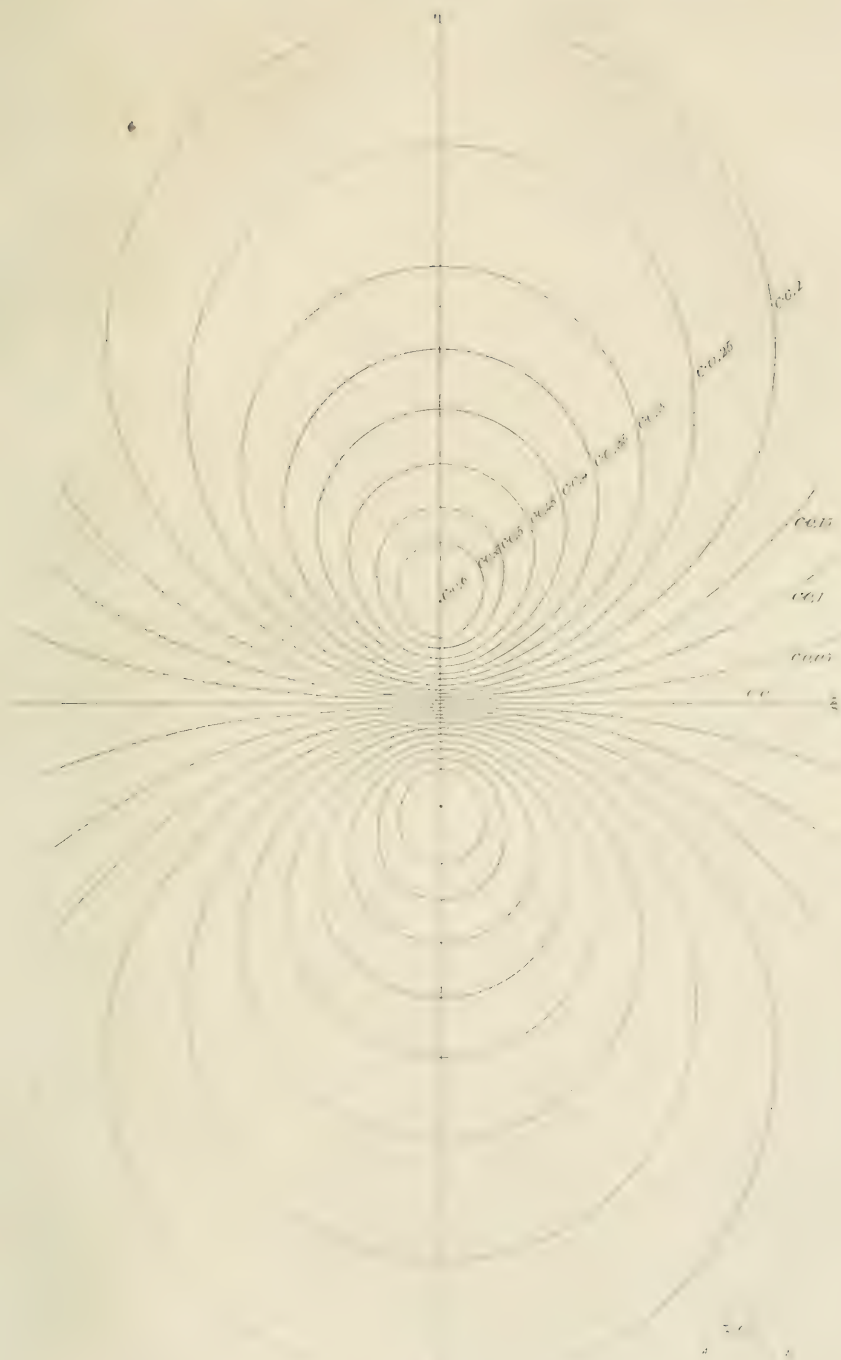
LXXVI. *On the Electric Currents induced by a Magnet in a Rotating Conductor.* By E. JOCHMANN†.

[With a Plate.]

THE earliest observed phenomenon in electric induction was, as is well known, the "rotation-magnetism" of Arago. Although the fundamental experiment, where a copper plate is made to rotate under the influence of a magnet, has been modified in a variety of ways, especially since Faraday's discovery of

\* Liebig's *Annalen*, January 1864.

† From the *Journal für die reine und angewandte Mathematik*, vol. lxiii. p. 158.







induced currents, upon which the true explanation of the phenomenon in question depends, our knowledge of the same, from an experimental as well as from a theoretical point of view, is still very deficient. The paths of the currents induced in the rotating conductor have by no means been experimentally determined with certainty; for, as I shall elsewhere show, the methods employed in investigating the curves described by these currents do not attain the object in view. These methods, in fact, are based upon the inadmissible assumption that the current-curves, and the lines of level of the potential of the free electricity, constitute two orthogonal systems. In a theoretic point of view, however, the treatment which the problem has hitherto received is still more imperfect, as will be evident from the following *résumé*. The laws of currents excited by magnetic induction in *linear* conductors were completely established by Neumann, Weber, and others, and reduced by Weber to the general principle of the mutual action of moving quantities of electricity. Kirchhoff\* afterwards exhibited the general differential equations for the variable electro-dynamic condition of stationary solid conductors, taking into consideration the inductive action produced between the several parts of the conductor by the variations in the intensity of the current; and he deduced from these equations certain general conclusions. With respect to the induction in solid conductors moving under the influence of a magnet, however, the general differential equations, upon whose integration the solution of the problem depends, have not as yet even been exhibited. Shortly after the publication of Arago's experiment, Poisson†, it is true, sought to explain the phenomenon by the action of magnetism at its moments of appearance and disappearance,—an action which might be very appreciable even in metals which, like copper, exert no action on the magnetic needle as soon as equilibrium in the magnetic condition of their molecules has been established. Since Faraday's discovery, however, Poisson's theory, notwithstanding its ingenuity and elaborate development, must be regarded as antiquated. Besides the experimental researches of Faraday‡, Nobili, Antinori§, and Matteucci||, to which I shall elsewhere return, a theoretical memoir

\* Poggendorff's *Annalen*, vol. cii. p. 529.

† "Sur la Théorie du Magnétisme en mouvement," *Mém. de l'Acad. des Sciences*, année 1823, vol. vi. (Paris, 1827).

‡ Phil. Trans. 1832.

§ *Antologia di Firenze*, No. 134; Poggendorff's *Annalen*, vol. xxiv. p. 621.

|| *Annales de Chimie et de Physique*, 3 sér. vol. xxxix; and *Cours spécial sur l'induction, le magnétisme de rotation*, &c. Paris, 1853.

by Felici\* must be here mentioned. Instigated by the observations of Matteucci, this author sought to determine the induction in an infinitely thin and unlimited plane disc, rotating under the influence of one or two opposite magnetic poles situated in the plane of the disc. Although, as will be afterwards shown, this case is only admissible as a limiting one, in consequence of the discontinuity therein involved, and although Felici, in solving the problem, employs several hypotheses the correctness of which requires proof, still, as far as the forms of the current-curves are concerned, the result of applying the following theory to this limiting case agrees with that obtained, in a quite different manner, by Felici himself.

In the following memoir I first exhibit the general equations of the motion of electricity in a conducting solid of revolution, of any form, which rotates with constant velocity around its axis, under the influence of a given magnetic distribution. The only assumptions required in the establishment and reduction of these equations are Weber's law of the mutual action of moving quantities of electricity, and the conditions which follow from the supposition of a constant electro-dynamic condition (*Strömungszustand*). Applying the equations to the particular case where the given magnetic distribution is symmetrical with respect to the axis of rotation, I then show that in this case the component of the current vanishes at every point of the conductor, consequently that no currents are induced, but that it is possible to assign a distribution of free electricity on the surface and in the interior of the conductor, such that its potential, at each point of the conductor, shall equilibrate with the electromotive force induced by the magnetism. I treat, lastly, the case of a disc of any thickness, bounded by two parallel planes, and rotating under the influence of one or more magnetic poles not situated in the axis of rotation. In so doing I assume, for the sake of simplification, that the velocity of rotation, and hence also the intensity of the currents, is sufficiently small to permit of our neglecting the inductive action between the several parts of the disc in presence of the direct inductive action of the magnetic poles.

1. When a homogeneous conducting solid of revolution rotates around its axis with constant velocity, under the influence of any given distribution of magnetic matter, or under the influence of any given system of closed galvanic currents external to the conductor, a system of currents is induced in the conductor, the direction and intensity of which at every point in space remains constant so long as the inducing magnetic force

\* Tortolini's *Annali di Scienze Matematiche e Fisiche*, 1853, p. 173; and 1854, p. 35.

and the velocity with which the conductor rotates remain unaltered. The electromotive force thus induced in any element of the conductor depends partly upon the relative motion of the element with respect to the given external magnetism, partly upon its motion relative to the system of currents existing in every other part of the conductor and fixed in space, and partly also upon the distribution of free electricity which ensues on the surface and also, as will be seen, on the interior of the conductor. Let  $u, v, w$  be the components, referred to three stationary rectangular coordinate axes, of the velocity with which an element at  $x, y, z$  within the conductor moves; further, let  $u, v, w$  be the components, referred to the same axes, of the intensity of the current at the point  $x, y, z$ ; and similarly let  $u', v', w'$  denote the components of the current in any other element of the conductor at  $x', y', z'$ . It will then follow from Weber's law\* that the electromotive force induced in the conductor-element, moving with the component velocities  $u, v, w$ , by the current-element  $u', v', w'$ , existing in the space-element  $dx' dy' dz'$ , is expressed by the formula

$$-\frac{2ki'\varpi}{r^2} \left( \cos \epsilon - \frac{3}{2} \cos \mathfrak{S} \cos \mathfrak{S}' \right) dx' dy' dz',$$

wherein

$$r = \sqrt{(x' - x)^2 + (y' - y)^2 + (z' - z)^2}$$

denotes the distance between the elements of the conductor and of the current;

$$i' = \sqrt{u'^2 + v'^2 + w'^2}$$

the density of the current in the point  $x', y', z'$ ;

$$\varpi = \sqrt{u^2 + v^2 + w^2}$$

the absolute velocity of the moving conductor-element;  $\epsilon, \mathfrak{S}$ , and  $\mathfrak{S}'$  respectively the angles between the directions of  $i'$  and  $\varpi$ , of  $\varpi$  and  $r$ , and of  $i'$  and  $r$ ; and lastly,

$$2k = \frac{1}{155370 \cdot 10^6}$$

the constant of induction referred to millimetres and seconds as units. Accordingly

\* *Electrodynamische Maassbestimmungen*, 1st memoir, p. 339; and 4th, p. 268. In the latter memoir the intensities of currents are estimated by electro-magnetic units of measures, whilst electromotive forces are referred to absolute mechanical units.

$$i' \varpi \cos \epsilon = uu' + vv' + ww',$$

$$i' \cos \vartheta = u' \frac{x' - x}{r} + v' \frac{y' - y}{r} + w' \frac{z' - z}{r},$$

$$\varpi \cos \vartheta = u \frac{x' - x}{r} + v \frac{y' - y}{r} + w \frac{z' - z}{r},$$

and the above expression becomes, by substitution,

$$- \frac{2k}{r^2} dx' dy' dz' \left[ uu' + vv' + ww' \right. \\ \left. - \frac{3}{2} \left( u \frac{x' - x}{r} + v \frac{y' - y}{r} + w \frac{z' - z}{r} \right) \left( u' \frac{x' - x}{r} + v' \frac{y' - y}{r} + w' \frac{z' - z}{r} \right) \right].$$

This is the force with which, through the action of the current-element  $u', v', w'$ , the units of positive and of negative electricity existing at  $x, y, z$  are separated in the direction of the connector  $r$ . By resolving this electromotive force into its three rectangular components, and taking the sum of the actions of all current-elements, we obtain the following expressions for the components of the electromotive action of the whole current-system on the unit of electricity at  $x, y, z$  :—

$$X = -2k \int \frac{dx' dy' dz'}{r^2} \cdot \frac{x' - x}{r} (uu' + vv' + ww') \\ + 3k \int \frac{dx' dy' dz'}{r^2} \cdot \frac{x' - x}{r} \left( u \frac{x' - x}{r} + v \frac{y' - y}{r} + w \frac{z' - z}{r} \right) \\ \left( u' \frac{x' - x}{r} + v' \frac{y' - y}{r} + w' \frac{z' - z}{r} \right),$$

$$Y = -2k \int \frac{dx' dy' dz'}{r^2} \cdot \frac{y' - y}{r} (uu' + vv' + ww') \\ + 3k \int \frac{dx' dy' dz'}{r^2} \cdot \frac{y' - y}{r} \left( u \frac{x' - x}{r} + v \frac{y' - y}{r} + w \frac{z' - z}{r} \right) \\ \left( u' \frac{x' - x}{r} + v' \frac{y' - y}{r} + w' \frac{z' - z}{r} \right),$$

$$Z = -2k \int \frac{dx' dy' dz'}{r^2} \cdot \frac{z' - z}{r} (uu' + vv' + ww') \\ + 3k \int \frac{dx' dy' dz'}{r^2} \cdot \frac{z' - z}{r} \left( u \frac{x' - x}{r} + v \frac{y' - y}{r} + w \frac{z' - z}{r} \right) \\ \left( u' \frac{x' - x}{r} + v' \frac{y' - y}{r} + w' \frac{z' - z}{r} \right).$$

These expressions admit of great simplification by introducing



the conditions to which  $u', v', w'$  are subject in the case of a constant electro-dynamic condition. For every point within the conductor we then have

$$\frac{\partial u'}{\partial x'} + \frac{\partial v'}{\partial y'} + \frac{\partial w'}{\partial z'} = 0, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and for every point of the surface of the conductor

$$u' \cos \lambda + v' \cos \mu + w' \cos \nu = 0, \quad . \quad . \quad (2)$$

where  $\lambda, \mu, \nu$  denote the angles between the positive directions of the coordinate axes and the normal to the surface of the conductor drawn inwards from the point  $x', y', z'$  on the same. The last equation expresses, of course, the condition under which the component of the current, perpendicular to the surface of the conductor, will vanish at every point of the same; whilst the expression on the left of the first equation represents the increment of negative electricity in the conductor-element  $dx', dy', dz'$ , which increment must likewise vanish on the hypothesis of a constant current-system. By introducing the values

$$\left. \begin{aligned} \alpha &= \int \frac{u'}{r} dx' dy' dz', \\ \beta &= \int \frac{v'}{r} dx' dy' dz', \\ \gamma &= \int \frac{w'}{r} dx' dy' dz', \end{aligned} \right\} . \quad . \quad . \quad . \quad . \quad (3)$$

the expression for  $X$  assumes the form

$$\begin{aligned} X &= -2k \left\{ u \frac{\partial \alpha}{\partial x} + v \frac{\partial \beta}{\partial x} + w \frac{\partial \gamma}{\partial x} \right\} \\ &+ 3k \left[ u \int \frac{(x' - x)^2}{r^5} \{ u'(x' - x) + v'(y' - y) + w'(z' - z) \} dx' dy' dz' \right. \\ &+ v \int \frac{(x' - x)(y' - y)}{r^6} \{ u'(x' - x) + v'(y' - y) + w'(z' - z) \} dx' dy' dz' \\ &\left. + w \int \frac{(x' - x)(z' - z)}{r^5} \{ u'(x' - x) + v'(y' - y) + w'(z' - z) \} dx' dy' dz' \right]. \end{aligned}$$

These integrals, it may be observed, have finite and determinate values, since the factors  $\frac{(x' - x)^2}{r^2}$ ,  $\frac{(x' - x)(y' - y)}{r^2}$ ,  $\frac{(x' - x)(z' - z)}{r^2}$  are severally less than unity, and moreover all integrals of the form

$$\int u' \cdot \frac{x' - x}{r} \cdot dx' dy' dz'$$

remain, as is well known, everywhere continuous and finite when extended to all points of the conductor.

The reduction of the integrals in the above expression may be effected on observing that

$$\begin{aligned} & 3 \int \frac{(x'-x)^2}{r^5} \{u'(x'-x) + v'(y'-y) + w'(z'-z)\} dx' dy' dz' \\ &= - \int \left\{ \frac{\partial \frac{u'(x'-x)^2}{r^3}}{\partial x'} + \frac{\partial \frac{v'(x'-x)^2}{r^3}}{\partial y'} + \frac{\partial \frac{w'(x'-x)^2}{r^3}}{\partial z'} \right\} dx' dy' dz' \\ &+ \int \frac{(x'-x)^2}{r^3} \left( \frac{\partial u'}{\partial x'} + \frac{\partial v'}{\partial y'} + \frac{\partial w'}{\partial z'} \right) dx' dy' dz' + 2 \int \frac{u'(x'-x)}{r^3} dx' dy' dz'. \end{aligned}$$

The second integral, on the right of the above equation-sign, vanishes in virtue of (1); the first, by performing one integration in each term and employing a known reduction-formula, becomes transformed into

$$- \int \frac{(x'-x)^2}{r^3} (u' \cos \lambda + v' \cos \mu + w' \cos \nu) ds,$$

and therefore, in virtue of the equation (2), also vanishes. The whole equation, therefore, assumes the form

$$\begin{aligned} & 3 \int \frac{(x'-x)^2}{r^5} \{u'(x'-x) + v'(y'-y) + w'(z'-z)\} dx' dy' dz' \\ &= 2 \int \frac{u'(x'-x)}{r^3} dx' dy' dz' = 2 \frac{\partial \alpha}{\partial x}. \end{aligned}$$

On treating in a similar manner the other constituents of X, we find

$$\begin{aligned} & 3 \int \frac{(x'-x)(y'-y)}{r^5} \{u'(x'-x) + v'(y'-y) + w'(z'-z)\} dx' dy' dz' \\ &= \frac{\partial \alpha}{\partial y} + \frac{\partial \beta}{\partial x}, \\ & 3 \int \frac{(x'-x)(z'-z)}{r^5} \{u'(x'-x) + v'(y'-y) + w'(z'-z)\} dx' dy' dz' \\ &= \frac{\partial \alpha}{\partial z} + \frac{\partial \gamma}{\partial x}. \end{aligned}$$

These values being substituted in the expression for X, and the components Y and Z being subjected to a similar treatment, the following results are obtained:—

$$\left. \begin{aligned} X &= k \left\{ v \left( \frac{\partial \alpha}{\partial y} - \frac{\partial \beta}{\partial x} \right) - w \left( \frac{\partial \gamma}{\partial x} - \frac{\partial \alpha}{\partial z} \right) \right\}, \\ Y &= k \left\{ w \left( \frac{\partial \beta}{\partial z} - \frac{\partial \gamma}{\partial y} \right) - u \left( \frac{\partial \alpha}{\partial y} - \frac{\partial \beta}{\partial x} \right) \right\}, \\ Z &= k \left\{ u \left( \frac{\partial \gamma}{\partial x} - \frac{\partial \alpha}{\partial z} \right) - v \left( \frac{\partial \beta}{\partial z} - \frac{\partial \gamma}{\partial y} \right) \right\}. \end{aligned} \right\} \quad \cdot \quad (4)$$

Before proceeding to the establishment of the corresponding expressions for the components of the electromotive forces produced by the given magnetic distribution, we shall examine a few properties of the integrals  $\alpha$ ,  $\beta$ ,  $\gamma$ , of which use will be subsequently made. Employing the general notation

$$\Delta \phi = \frac{\partial^2 \phi}{\partial x^2} + \frac{\partial^2 \phi}{\partial y^2} + \frac{\partial^2 \phi}{\partial z^2},$$

it is manifest that, for every point within the conductor,

$$\Delta \alpha = -4\pi u, \quad \Delta \beta = -4\pi v, \quad \Delta \gamma = -4\pi w. \quad \cdot \quad (5)$$

Further,

$$\begin{aligned} \frac{\partial \alpha}{\partial x} + \frac{\partial \beta}{\partial y} + \frac{\partial \gamma}{\partial z} &= \int \frac{u'(x'-x) + v'(y'-y) + w'(z'-z)}{r^3} dx' dy' dz' \\ &= - \int \left\{ \frac{\partial}{\partial x'} \frac{u'}{r} + \frac{\partial}{\partial y'} \frac{v'}{r} + \frac{\partial}{\partial z'} \frac{w'}{r} \right\} dx' dy' dz' \\ &\quad + \int \frac{1}{r} \left( \frac{\partial u'}{\partial x'} + \frac{\partial v'}{\partial y'} + \frac{\partial w'}{\partial z'} \right) dx' dy' dz', \end{aligned}$$

both of which last integrals vanish in consequence of the equations (1) and (2); so that

$$\frac{\partial \alpha}{\partial x} + \frac{\partial \beta}{\partial y} + \frac{\partial \gamma}{\partial z} = 0. \quad \cdot \quad \cdot \quad \cdot \quad (6)$$

In consequence of this relation (6), the equations (5) assume the forms

$$\left. \begin{aligned} \frac{\partial}{\partial} \left( \frac{\partial \gamma}{\partial x} - \frac{\partial \alpha}{\partial z} \right) - \frac{\partial}{\partial y} \left( \frac{\partial \alpha}{\partial y} - \frac{\partial \beta}{\partial x} \right) &= 4\pi u, \\ \frac{\partial}{\partial x} \left( \frac{\partial \alpha}{\partial y} - \frac{\partial \beta}{\partial x} \right) - \frac{\partial}{\partial z} \left( \frac{\partial \beta}{\partial z} - \frac{\partial \gamma}{\partial y} \right) &= 4\pi v, \\ \frac{\partial}{\partial y} \left( \frac{\partial \beta}{\partial z} - \frac{\partial \gamma}{\partial y} \right) - \frac{\partial}{\partial x} \left( \frac{\partial \gamma}{\partial x} - \frac{\partial \alpha}{\partial z} \right) &= 4\pi w. \end{aligned} \right\} \quad \cdot \quad (7)$$

2. In order to exhibit the differential equations of the motion of electricity, we have now merely to determine the electromotive forces originated by the given magnetic distribution outside the

conductor; for, of course, any additional system of closed currents may always be replaced by a distribution of magnetism.

According to Weber's law\*, the electromotive force induced by the magnetic element  $\mu$  in the conductor-element at  $x, y, z$ , moving with the velocity  $\varpi = \sqrt{u^2 + v^2 + w^2}$ , is equal to

$$2k\varpi \frac{\mu \sin \phi}{r^2};$$

in which expression  $r$  denotes the distance between the elements,  $k$  the constant of magnetic induction, and  $\phi$  the angle enclosed by the directions of  $r$  and  $\varpi$ . The constant  $k$  will be identical with the one previously represented by this letter, provided the intensities of all currents be measured by electro-magnetic units. The direction in which this electromotive force strives to separate the electricities in the element at  $x, y, z$ , is perpendicular to the plane through  $r$  and the direction in which this element moves. Consequently, if  $\lambda, \mu, \nu$  be the direction-angles of the electromotive force,

$$\begin{cases} (x' - x) \cos \lambda + (y' - y) \cos \mu + (z' - z) \cos \nu = 0, \\ u \cos \lambda + v \cos \mu + w \cos \nu = 0, \\ \cos^2 \lambda + \cos^2 \mu + \cos^2 \nu = 1. \end{cases}$$

The equations give at once the values

$$\varpi \cos \lambda = \frac{v(z' - z) - w(y' - y)}{r \sin \phi},$$

$$\varpi \cos \mu = \frac{w(x' - x) - u(z' - z)}{r \sin \phi},$$

$$\varpi \cos \nu = \frac{u(y' - y) - v(x' - x)}{r \sin \phi}.$$

Accordingly the rectangular components of the electromotive force at  $x, y, z$  induced by the magnetic particle  $\mu$  at  $x', y', z'$  are

$$2k\mu \left\{ v \frac{z' - z}{r^3} - w \frac{y' - y}{r^3} \right\} = 2k \left\{ v \frac{\partial \frac{\mu}{r}}{\partial z} - w \frac{\partial \frac{\mu}{r}}{\partial y} \right\},$$

$$2k\mu \left\{ w \frac{x' - x}{r^3} - u \frac{z' - z}{r^3} \right\} = 2k \left\{ w \frac{\partial \frac{\mu}{r}}{\partial x} - u \frac{\partial \frac{\mu}{r}}{\partial z} \right\},$$

$$2k\mu \left\{ u \frac{y' - y}{r^3} - v \frac{x' - x}{r^3} \right\} = 2k \left\{ u \frac{\partial \frac{\mu}{r}}{\partial y} - v \frac{\partial \frac{\mu}{r}}{\partial x} \right\}.$$

\* *Electrodynamische Maassbestimmungen*, 1st memoir, p. 345.



Hence if  $\mu dx' dy' dz'$  denote the quantity of magnetic fluid contained in the volume-element  $dx' dy' dz'$  outside the conductor, and

$$P = \int \frac{\mu}{r} dx' dy' dz' \quad . \quad . \quad . \quad . \quad (8)$$

the potential of the given magnetic distribution, the required components of the total electromotive action at the point  $x, y, z$  will be

$$A = 2k \left\{ v \frac{\partial P}{\partial z} - w \frac{\partial P}{\partial y} \right\},$$

$$B = 2k \left\{ w \frac{\partial P}{\partial x} - u \frac{\partial P}{\partial z} \right\},$$

$$C = 2k \left\{ u \frac{\partial P}{\partial y} - v \frac{\partial P}{\partial x} \right\}.$$

Since, by hypothesis, there is no magnetic matter within the rotating conductor, we have, by a well-known theorem,

$$\Delta P = 0. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

3. If  $V$  be the potential of the free electricity existing on the rotating conductor, and  $K$  the conductivity of the latter, the components of the current-density at the point  $x, y, z$  will be

$$\left. \begin{aligned} u &= K \left\{ -\frac{\partial V}{\partial x} + X + A \right\}, \\ v &= K \left\{ -\frac{\partial V}{\partial y} + Y + B \right\}, \\ w &= K \left\{ -\frac{\partial V}{\partial z} + Z + C \right\}. \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (10)$$

Putting

$$\left. \begin{aligned} L &= 2k \frac{\partial P}{\partial x} + k \left( \frac{\partial \beta}{\partial z} - \frac{\partial \gamma}{\partial y} \right), \\ M &= 2k \frac{\partial P}{\partial y} + k \left( \frac{\partial \gamma}{\partial x} - \frac{\partial \alpha}{\partial z} \right), \\ N &= 2k \frac{\partial P}{\partial z} + k \left( \frac{\partial \alpha}{\partial y} - \frac{\partial \beta}{\partial x} \right), \end{aligned} \right\} \quad . \quad . \quad . \quad (11)$$

we have

$$\left. \begin{aligned} X + A &= v N - w M, \\ Y + B &= w L - u N, \\ Z + C &= u M - v L, \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (12)$$

from which expressions we deduce immediately the relation

$$u(X + A) + v(Y + B) + w(Z + C) = 0,$$

and from the same conclude that the total electromotive force arising from the several inductive actions is everywhere perpendicular to the direction in which the conductor-element moves. By differentiating the expressions (11) and employing (7) and (9), we deduce the relations

$$\left. \begin{aligned} \frac{\partial M}{\partial z} - \frac{\partial N}{\partial y} &= 4\pi k u, \\ \frac{\partial N}{\partial x} - \frac{\partial L}{\partial z} &= 4\pi k v, \\ \frac{\partial L}{\partial y} - \frac{\partial M}{\partial x} &= 4\pi k w, \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (13)$$

$$\frac{\partial L}{\partial x} + \frac{\partial M}{\partial y} + \frac{\partial N}{\partial z} = 0. \quad . \quad . \quad . \quad . \quad (14)$$

On substituting the values (12), the equations (10) assume the forms

$$\left. \begin{aligned} u &= K \left\{ -\frac{\partial V}{\partial x} + \mathfrak{b} N - \mathfrak{w} M \right\}, \\ v &= K \left\{ -\frac{\partial V}{\partial y} + \mathfrak{w} L - u N \right\}, \\ w &= K \left\{ -\frac{\partial V}{\partial z} + u M - \mathfrak{b} L \right\}. \end{aligned} \right\} \quad . \quad . \quad (15)$$

From these three equations, in combination with the condition

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0, \quad . \quad . \quad . \quad . \quad (16)$$

which is to be fulfilled at every point within the conductor, the functions  $u$ ,  $v$ ,  $w$ , and  $V$  must be so determined that, at the same time, the equation

$$u \cos \lambda + v \cos \mu + w \cos \nu = 0 \quad . \quad . \quad . \quad (17)$$

will be satisfied at every point of the surface. By means of the equations (13), we may eliminate from the equations (15) either  $u$ ,  $v$ ,  $w$  or  $L$ ,  $M$ ,  $N$ . In the first case we obtain

$$\left. \begin{aligned} \frac{\partial M}{\partial z} - \frac{\partial N}{\partial y} &= 4\pi k K \left\{ -\frac{\partial V}{\partial x} + \mathfrak{b} N - \mathfrak{w} M \right\}, \\ \frac{\partial N}{\partial x} - \frac{\partial L}{\partial z} &= 4\pi k K \left\{ -\frac{\partial V}{\partial y} + \mathfrak{w} L - u N \right\}, \\ \frac{\partial L}{\partial y} - \frac{\partial M}{\partial x} &= 4\pi k K \left\{ -\frac{\partial V}{\partial z} + u M - \mathfrak{b} L \right\}, \end{aligned} \right\} \quad . \quad (18)$$

from which equations, in combination with (14), the functions  $L, M, N, V$  are to be so determined that the surface-condition

$$\left(\frac{\partial M}{\partial z} - \frac{\partial N}{\partial y}\right) \cos \lambda + \left(\frac{\partial N}{\partial x} - \frac{\partial L}{\partial z}\right) \cos \mu + \left(\frac{\partial L}{\partial y} - \frac{\partial M}{\partial x}\right) \cos \nu = 0 \quad (19)$$

shall be simultaneously satisfied. It should here be remarked that, since the equations (13) result from the differentiation of (11), the former are necessary consequences of the latter; the converse of this, however, is not true. The equations (18) therefore express the possibility of a more general motion of electricity than do the original equations (15), so that the former must always be satisfied by integrals of the latter, but not conversely. Consequently the arbitrary functions involved in any general integrals of the equations (18) will have to be so specialized that the equations (11) may be simultaneously fulfilled. For instance, the integral equations  $L=0, M=0, N=0$ , which imply also  $u=0, v=0, w=0$ , constitute a particular solution of the equations (18); not being consistent with the equations (11), however, this solution cannot be regarded as a solution of the equations (15) unless  $P=\text{const.}$  A system of equations for  $u, v, w$ , exactly similar in form to the system (18) for  $L, M, N$ , is obtained when, conversely,  $L, M, N$  are eliminated from (15) by means of the equations (13). In fact, if by differentiation of (15) we form  $\frac{\partial v}{\partial z} - \frac{\partial w}{\partial y}$  and the other analogous expressions, and put

$$\Psi = -\frac{1}{4\pi k}(uL + vM + wN),$$

we shall find for the determination of the functions  $u, v, w, \Psi$  the equations

$$\left. \begin{aligned} \frac{\partial v}{\partial z} - \frac{\partial w}{\partial y} &= 4\pi k K \left\{ -\frac{\partial \Psi}{\partial x} + vw - wv \right\}, \\ \frac{\partial w}{\partial x} - \frac{\partial u}{\partial z} &= 4\pi k K \left\{ -\frac{\partial \Psi}{\partial y} + wu - uw \right\}, \\ \frac{\partial u}{\partial y} - \frac{\partial v}{\partial x} &= 4\pi k K \left\{ -\frac{\partial \Psi}{\partial z} + uv - vu \right\}, \\ \frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} &= 0, \end{aligned} \right\} \quad (20)$$

to which the same remark applies as was previously made with reference to the equations (18). A knowledge of  $u, v, w$  is not necessary when the object is merely to determine the action exerted upon an external magnetic pole by the system of cur-

rents induced in the conductor, since, with the exception of a constant factor, the second terms in the expressions for  $L, M, N$  give obviously the rectangular components of this action upon an external point.

If we take the axis of rotation for axis of  $Z$ , and denote by  $n$  the angular velocity of rotation, we have

$$u = -ny, \quad v = nx, \quad w = 0,$$

and the equations (15) become transformed thus :

$$\left. \begin{aligned} u &= K \left\{ -\frac{\partial V}{\partial x} + nxN \right\}, \\ v &= K \left\{ -\frac{\partial V}{\partial y} + nyN \right\}, \\ w &= K \left\{ -\frac{\partial V}{\partial z} - n(xL + yM) \right\}. \end{aligned} \right\} \dots (15^*)$$

\*       \*       \*

4. In the particular case when the given magnetic distribution is symmetrical with respect to the axis of rotation—in other words, when  $P$  is a function of  $z$ , and  $r = \sqrt{x^2 + y^2}$ —the equations (15\*) may be satisfied by assuming

$$u = 0, \quad v = 0, \quad w = 0.$$

That is to say, a distribution of free electricity within and upon the surface of the conductor may always be assigned so that its potential shall at every point of the conductor equilibrate with the electromotive force induced by the magnetic distribution, and thus prevent the production of currents. In this case, in fact, the expressions (11) are reducible to

$$L = 2k \frac{\partial P}{\partial x}, \quad M = 2k \frac{\partial P}{\partial y}, \quad N = 2k \frac{\partial P}{\partial z},$$

whereby the equations (15\*) become

$$\left. \begin{aligned} \frac{\partial V}{\partial x} &= 2nkx \frac{\partial P}{\partial z}, \\ \frac{\partial V}{\partial y} &= 2nky \frac{\partial P}{\partial z}, \\ \frac{\partial V}{\partial z} &= -2nk \left( x \frac{\partial P}{\partial x} + y \frac{\partial P}{\partial y} \right). \end{aligned} \right\} \dots (21)$$

It is readily seen that when  $P$  is a function of  $r$  and  $z$  alone, the expressions on the right of these equations satisfy the conditions of integrability of the system. By the introduction of polar



coordinates, the equations assume the forms

$$\left. \begin{aligned} \frac{\partial V}{\partial r} &= 2nkr \frac{\partial P}{\partial z}, \\ \frac{\partial V}{\partial z} &= -2nkr \frac{\partial P}{\partial r}. \end{aligned} \right\} \dots \dots \dots (22)$$

Since  $P$ , as a potential function of masses external to the conductors, satisfies the equation

$$\frac{\partial \left( r \frac{\partial P}{\partial r} \right)}{\partial r} + r \frac{\partial^2 P}{\partial z^2} = 0,$$

we deduce the value

$$V = 2nk \int \left( r \frac{\partial P}{\partial z} dr - r \frac{\partial P}{\partial r} dz \right). \dots \dots (23)$$

The equations (21), differentiated respectively according to  $x, y, z$ , give

$$\Delta V = 4nk \frac{\partial P}{\partial z} = -4\pi\epsilon, \dots \dots \dots (24)$$

which determines the density  $\epsilon$  of the free electricity at every point within the conductor. In order to find the density  $e$  at any point of the conductor's surface, we have to remember that by the equation (23) the value of  $V$  is given for every point of this surface, that throughout the external space  $\Delta V = 0$ , and that at infinity  $V$  must vanish. These conditions determine the value of  $V_a$  throughout external space, and by a known relation we have

$$\left( \frac{\partial V}{\partial N} \right)_a - \left( \frac{\partial V}{\partial N} \right)_i = -4\pi e. \dots \dots \dots (25)$$

For a sphere rotating under the influence of a constant magnetic force, whose direction coincides with the axis of rotation,

$$\frac{\partial P}{\partial z} = T = \text{const.};$$

whence

$$V_i = nkTr^2 + \text{const.}$$

Consequently,  $R$  being the radius of the sphere, and  $\vartheta$  the angle which the radius forms with the  $Z$ -axis, we obtain for each point of the spherical surface the value

$$\bar{V} = nkTR^2 \sin^2 \vartheta + C.$$

By well-known methods we find, further, that

$$V_a = \left( C + \frac{2nkTR^2}{3} \right) \frac{R}{\rho_a} - \left( \cos^2 \vartheta - \frac{1}{3} \right) nkTR^2 \left( \frac{R}{\rho_a} \right)^3,$$

where  $\rho_a$  denotes the distance of any external point from the centre of the sphere; and for an internal point, that

$$V_i = nkT\rho_i^2 \sin^2 \vartheta + C.$$

Hence, since

$$e = -\frac{1}{4\pi} \left[ \left( \frac{\partial V_a}{\partial \rho} \right)_{\rho=R} - \left( \frac{\partial V_i}{\partial \rho} \right)_{\rho=R} \right],$$

we have

$$e = \frac{C}{4\pi R} + \frac{nkTR}{2\pi} \left( \frac{11}{6} - \frac{5}{2} \cos^2 \vartheta \right).$$

Such is the density of the electricity on the surface of the sphere, the interior being at the same time filled with free electricity having the constant density

$$\epsilon = -\frac{nkT}{\pi}.$$

The constant  $C$  is determined from the arbitrary total quantity of free electricity which exists on the sphere. In fact the quantity within the sphere is

$$-\frac{4}{3}nkTR^3,$$

and the quantity spread over the surface is

$$CR + 2nkTR^3.$$

Consequently were the sum of all the quantities of electricity to vanish, we should have

$$C = -\frac{2}{3}nkTR^2,$$

and hence

$$e = \frac{nkTR}{2\pi} \left\{ \frac{5}{2} \sin^2 \vartheta - 1 \right\}.$$

When a solid of revolution of any form rotates around its axis under the influence of a magnetic pole  $\mu$ , situated thereon at the distance  $c$  from the origin of coordinates, we have

$$P = \frac{\mu}{\sqrt{r^2 + (c-z)^2}},$$

$$V_i = \frac{2nk\mu(c-z)}{\sqrt{r^2 + (c-z)^2}} + \text{const.}$$

If  $\gamma$  denote the angle made with the axis of rotation by the connector of the magnetic pole and the point of the conductor under consideration, the last equation may be written thus:

$$V_i = -2nk\mu \cos \gamma + \text{const.}$$

Further,  $\bar{V}_1$  and  $\bar{V}_2$  being the values of the potential of free electricity at any two points of the surface of the conductor, it follows that

$$\bar{V}_1 - \bar{V}_2 = -2nk\mu(\cos \gamma_1 - \cos \gamma_2).$$

If these two points be connected by a stationary conducting wire of considerable resistance, the latter will be traversed by a current whose intensity will be proportional to the difference of the values of the potential, and therefore to the difference of the cosines\*. Now, according to a formula given by Green, we have in the case of the sphere,

$$V_a = \frac{\rho^2 - R^2}{4\pi R} \iint \bar{V} \frac{dS}{r^3},$$

where  $\bar{V}$  denotes the value of  $V$  at the element  $dS$  of the spherical surface,  $\rho$  the distance of the external point from the sphere's centre, and  $r$  its distance from the element  $dS$ . The integration is to be extended, of course, to all points of the surface of the sphere. In the case under consideration,

$$V_a = -nk\mu \frac{\rho^2 - R^2}{2\pi R} \iint \frac{\cos \gamma}{r^3} dS.$$

\* \* \* \* \*

5. The solution of the general problem is considerably simplified by the assumption of a velocity of rotation so small that the intensity of the induced currents is sufficiently weak to justify our neglecting, in presence of the direct inductive action of the magnet, the induction which takes place between the several parts of the conductor†. By this assumption the expressions (11) are reduced to their first terms. For if  $n$  denote, as before, the velocity of rotation, the effective electromotive forces as well as the generated current-densities  $u$ ,  $v$ ,  $w$  will be magnitudes of the order  $nk$ . Consequently the expressions within brackets which, multiplied by  $k$ , constitute the second terms in the formulæ for  $L$ ,  $M$ ,  $N$  will also be of the order  $nk$ ; whilst in the equations (15\*) those portions of the expressions  $nL$ ,  $nM$ ,  $nN$  which proceed from the terms in question will be magnitudes of the order  $n^2k^2$ . Even when the velocity of rotation is tolerably great, the latter terms may, without incurring appreciable error, be neglected in presence of the terms of the order  $nk$ , seeing that the value of the constant of induction  $k$  is

\* Weber's unipolar induction.

† This simplifying assumption has in fact been tacitly made in all previous investigations in connexion with the subject.

very small. This done, the equations (15\*) are reduced to the following :—

$$\left. \begin{aligned} u &= K \left\{ -\frac{\partial V}{\partial x} + 2nkx \frac{\partial P}{\partial z} \right\}, \\ v &= K \left\{ -\frac{\partial V}{\partial y} + 2nky \frac{\partial P}{\partial z} \right\}, \\ w &= K \left\{ -\frac{\partial V}{\partial z} - 2nk \left( x \frac{\partial P}{\partial x} + y \frac{\partial P}{\partial y} \right) \right\}; \end{aligned} \right\} \quad (26)$$

and the equation (16) becomes transformed thus :

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2} = 4nk \frac{\partial P}{\partial z} \quad \dots \quad (27)$$

This equation, therefore, has merely to be integrated, subject to the limiting condition

$$\left. \begin{aligned} \frac{\partial V}{\partial x} \cos \lambda + \frac{\partial V}{\partial y} \cos \mu + \frac{\partial V}{\partial z} \cos \nu \\ = 2nk(x \cos \lambda + y \cos \mu) \frac{\partial P}{\partial z} - 2nk \cos \nu \left( x \frac{\partial P}{\partial x} + y \frac{\partial P}{\partial y} \right); \end{aligned} \right\} \quad (28)$$

after which  $u, v, w$  will be found by simple differentiation.

As an example, we will treat the case of a disc of arbitrary thickness, bounded by two parallel planes, which rotates, under the influence of an external magnetic pole, around an axis perpendicular to those planes. Let  $2\delta$  be the thickness of the disc,

$$z = \delta \text{ and } z = -\delta$$

the equations of the limiting planes, and  $a, b, c$  the coordinates of the inducing pole, so that

$$P = \frac{\mu}{\rho}, \quad \dots \quad (29)$$

provided

$$\rho = \sqrt{(x-a)^2 + (y-b)^2 + (z-c)^2}$$

denote the distance of this pole from any point of the disc.

The limiting condition will be fulfilled if, for  $z = \pm \delta$ ,

$$w = 0,$$

or

$$\frac{\partial V}{\partial z} = -2nk \left( x \frac{\partial P}{\partial x} + y \frac{\partial P}{\partial y} \right). \quad \dots \quad (28^*)$$

We will now show that the equations (26) and (27) may be satisfied by assuming that everywhere  $w=0$ ,—in other words, that the currents flow in planes parallel to the two by which the



disc is bounded. Since

$$\frac{\partial^2 V}{\partial z^2} = -2nk \frac{\partial}{\partial z} \left( x \frac{\partial P}{\partial x} + y \frac{\partial P}{\partial y} \right),$$

it follows from (27) that

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} = 2nk \frac{\partial}{\partial z} \left\{ 2P + x \frac{\partial P}{\partial x} + y \frac{\partial P}{\partial y} \right\}. \quad (30)$$

This equation is satisfied by the value

$$V = 2nk\mu \frac{\partial W}{\partial z}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

provided  $W$  be understood to be a solution of the equation

$$\frac{\partial^2 W}{\partial x^2} + \frac{\partial^2 W}{\partial y^2} = \frac{2}{\rho} - \frac{x(x-a) + y(y-b)}{\rho^3},$$

and the value of  $P$  be taken into consideration. On observing that

$$\frac{\partial^2 \rho}{\partial x^2} + \frac{\partial^2 \rho}{\partial y^2} = \frac{2}{\rho} - \frac{(x-a)^2 + (y-b)^2}{\rho^3},$$

we deduce at once the relation

$$\frac{\partial^2 (W - \rho)}{\partial x^2} + \frac{\partial^2 (W - \rho)}{\partial y^2} = - \frac{a(x-a) + b(y-b)}{\rho^3}. \quad (32)$$

If, now, for simplicity, the origin of coordinates be removed to the inducing pole by the substitutions

$$x-a=\xi, \quad y-b=\eta, \quad z-c=\zeta,$$

it will be readily seen that to the equation (32) corresponds the integral

$$W = \rho + \frac{a\xi + b\eta}{\rho \pm \zeta}, \quad . \quad . \quad . \quad . \quad . \quad (33)$$

whence we deduce the value

$$V = 2nk\mu \frac{\partial W}{\partial z} = 2nk\mu \left\{ -\frac{\zeta}{\rho} \pm \frac{a\xi + b\eta}{\rho(\rho \pm \zeta)} \right\}.$$

The upper sign is alone admissible in the present problem, since  $V$  must possess the characteristic properties of a potential function, and consequently remain finite and continuous throughout the interior of the conductor, and vanish, together with its differential quotients, when  $\xi = \infty$  or  $\eta = \infty$ . For it is easy to see that the value

$$V = 2nk\mu \left\{ -\frac{\zeta}{\rho} + \frac{a\xi + b\eta}{\rho(\rho + \zeta)} \right\} \quad . \quad . \quad . \quad . \quad (34)$$

not only satisfies at every internal point the equations (27) and

(28\*), but that it also possesses the above-mentioned properties so long as  $\zeta$  differs from zero,—that is to say, so long as the inducing pole remains at a finite distance from the limiting surface of the conductor. On the other hand, the value of  $V$  which corresponds to the negative sign is discontinuous when  $\xi=0$  and  $\eta=0$ .

Taking into consideration the equations (29) and (31), it follows from (26) that

$$u = 2nk\mu K \frac{\partial}{\partial z} \left( \frac{x}{\rho} - \frac{\partial W}{\partial x} \right) = 2nk\mu K \frac{\partial}{\partial z} \left( \frac{a}{\rho} - \frac{\partial}{\partial x} \frac{a\xi + b\eta}{\rho + \zeta} \right),$$

$$v = 2nk\mu K \frac{\partial}{\partial z} \left( \frac{y}{\rho} - \frac{\partial W}{\partial y} \right) = 2nk\mu K \frac{\partial}{\partial z} \left( \frac{b}{\rho} - \frac{\partial}{\partial y} \frac{a\xi + b\eta}{\rho + \zeta} \right);$$

which values may obviously be written thus:

$$u = 2nk\mu K \frac{\partial}{\partial z} \left( \frac{\partial}{\partial y} \frac{a\eta - b\xi}{\rho + \zeta} \right) = 2nk\mu K \frac{\partial}{\partial y} \frac{a\eta - b\xi}{\rho(\rho + \zeta)},$$

$$v = 2nk\mu K \frac{\partial}{\partial z} \left( \frac{\partial}{\partial x} \frac{b\xi - a\eta}{\rho + \zeta} \right) = 2nk\mu K \frac{\partial}{\partial x} \frac{b\xi - a\eta}{\rho(\rho + \zeta)};$$

or, by reintroducing the coordinates  $x, y, z$ , thus:

$$\left. \begin{aligned} u &= 2nk\mu K \frac{\partial}{\partial y} \frac{ay - bx}{\rho(\rho + c - z)}, \\ v &= 2nk\mu K \frac{\partial}{\partial x} \frac{bx - ay}{\rho(\rho + c - z)}. \end{aligned} \right\} \dots \dots (35)$$

The differential equation of the *current-curves* is consequently

$$vdx - udy = 0,$$

which by integration gives

$$\frac{bx - ay}{\rho(\rho + c - z)} = \text{const.} \dots \dots (36)$$

The differential equation for  $V$  being linear, the solution of the problem contained in the equations (34), (35), and (36) may be at once extended to any number of poles whatever. We have, in fact,

$$V = 2nk \sum \mu \left\{ -\frac{c-z}{\rho} + \frac{a(x-a) + b(y-b)}{\rho(\rho + c - z)} \right\}, \quad (34^*)$$

$$\left. \begin{aligned} u &= -2nkK \frac{\partial \Psi}{\partial y}, \\ v &= +2nkK \frac{\partial \Psi}{\partial x}, \end{aligned} \right\} \dots \dots \dots (35^*)$$

provided we make

$$\Psi = \Sigma \frac{\mu(bx - ay)}{\rho(\rho + c - z)},$$

in which case

$$\Psi = \text{const.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36^*)$$

will be the equation of the current-curves.

6. In the case of a *single* inducing pole, the axis of  $X$  may for simplicity be made to pass through the same so that  $b=0$ ; and on introducing the coordinates  $\xi, \eta, \zeta$ ,

$$\left. \begin{aligned} u &= 2nk\mu Ka \frac{\partial}{\partial \eta} \frac{\eta}{\rho(\rho+\xi)} = 2nk\mu Ka \frac{\rho^2(\rho+\xi) - \eta^2(2\rho+\xi)}{\rho^3(\rho+\xi)^2}, \\ v &= -2nk\mu Ka \frac{\partial}{\partial \xi} \frac{\eta}{\rho(\rho+\xi)} = 2nk\mu Ka \frac{\xi\eta(2\rho+\xi)}{\rho^3(\rho+\xi)^2}, \end{aligned} \right\} (37)$$

wherein  $a$  denotes the distance of the pole from the axis of rotation. The equation of the current-curves is now

$$\frac{\eta}{\rho(\rho + \xi)} = C. \quad . \quad . \quad . \quad . \quad . \quad (38)$$

A noteworthy result, immediately deducible from this, is that *the form of the current-curves induced by a single pole is independent of the distance of the pole from the axis of rotation*; whilst, as shown by the expressions (37), the current-density is proportional to this distance, and vanishes when  $a=0$ . The equation (38) represents a system of curves of the fourth order, of which, however, only those branches correspond to the problem for which

$$\rho = \sqrt{\xi^2 + \eta^2 + \zeta^2}$$

has a positive value. The currents are situated in planes parallel to the two by which the disc is bounded; the parameter  $\zeta$  characterizes the whole group of current-curves which belong to the plane  $\zeta=c-z$ ; whilst the constant  $C$  changes from one curve to another in the same plane. The system of curves corresponding to one and the same plane, constructed according to the equation (38), is represented in Plate V., wherein the distance of the inducing pole from the plane of the figure is shown by the line  $AB$ , whose length, according to the arbitrary scale adopted, is equal to 0.5.

Since  $v$  vanishes for  $\eta=0$ , it follows that no curve cuts the axis of  $\xi$ . The denominator of the expression (38) being always positive, the corresponding values of  $\eta$  and the constant  $C$  are

either both positive or both negative. When  $C=0$ , then  $\eta=0$  for all values of  $\xi$ , and the curve corresponding to this value of  $C$  coincides with the axis of  $\xi$ . The curves corresponding to equal and opposite values of  $C$  are symmetrical with respect to the axis of  $\xi$ . Every curve is symmetrical also with respect to the axis of  $\eta$ , since its equation involves  $\xi^2$  solely. All curves, with the exception of the one for which  $C=0$ , are closed; for since for real values of  $\xi$  the fraction  $\frac{\eta}{\rho}$  is always a proper one,

the value of the quotient (38) must diminish, when  $\rho$  increases, beyond any assignable magnitude. It is also manifest that for a given value of  $C$ , the ordinate  $\eta$  cannot sink below a determinate limit. The greatest and smallest admissible values of  $\eta$  and  $\rho$  correspond to  $\xi=0$ ; consequently every curve cuts the axis of  $\eta$  perpendicularly in two real points, and all curves circulate around two points situated upon this axis at equal distances from the axis of  $\xi$ . These points, which for brevity we will call *whirling-points* (Wirbelpunkte), correspond, as curves of the system, to a maximum value of  $C$ , or to  $u=0$  and  $v=0$ . Their coordinates are given by the equations

$$\begin{aligned}\xi\eta(2\rho + \zeta) &= 0, \\ \rho^2(\rho + \zeta) &= \eta^2(2\rho + \zeta).\end{aligned}$$

Hence, since  $\rho$  is always positive,  $\eta$  according to the second equation can never vanish. From the first equation, therefore, it follows that  $\xi=0$ , and hence  $\rho^2=\eta^2+\zeta^2$ ; so that the second equation takes ultimately the form

$$\eta^4 - \eta^2\zeta^2 - \zeta^4 = 0,$$

whose real roots are

$$\eta = \pm \sqrt{\frac{1 + \sqrt{5}}{2}}.$$

The maximum value of  $C$ , corresponding to the whirling-points, is

$$C = \pm \frac{\zeta^4}{\eta^5} = \pm \frac{1}{\zeta} \sqrt{\frac{2}{11 + 5\sqrt{5}}}.$$

To greater positive or negative values of  $C$  correspond no real current-curves.

The whirling-points of the current-systems situated in the several planes lie on two right lines which intersect in the inducing pole, and enclose an angle of about  $103^\circ 39'$ . In the limiting case, where the pole approaches infinitely close to the surface of the disc, that is to say, when  $\zeta=0$  is the equation of one of the limiting planes of the latter, the two whirling-points approach



one another indefinitely, and the current-curves degenerate to a system of circles

$$\eta = C(\xi^2 + \eta^2)$$

having the axis of  $\xi$  for a common tangent. This is the same system of circles as that which Felici found in the case where the pole was situated in the plane of an infinitely thin disc. It must, however, be observed that this case is only admissible as a limiting one, since the electromotive force, which is proportional to  $\frac{\partial P}{\partial z}$ , is equal to zero for all points of the disc, with the exception of the point  $\xi=0, \eta=0$ , where it, together with the potential  $V$  and the current-intensity, becomes infinite.

The components of the action of a system of closed currents on a magnetic pole  $m$  situated at a point  $x, y, z$  external to the conductor are given by the known formulæ

$$L' = m \int \frac{v'(z' - z) - w'(y' - y)}{r^3} dx' dy' dz',$$

$$M' = m \int \frac{w'(x' - x) - u'(z' - z)}{r^3} dx' dy' dz',$$

$$N' = m \int \frac{u'(y' - y) - v'(x' - x)}{r^3} dx' dy' dz',$$

which are expressible thus :

$$L' = m \left( \frac{\partial \beta}{\partial z} - \frac{\partial \gamma}{\partial y} \right), \quad M' = m \left( \frac{\partial \gamma}{\partial x} - \frac{\partial \alpha}{\partial z} \right), \quad N' = m \left( \frac{\partial \alpha}{\partial y} - \frac{\partial \beta}{\partial x} \right),$$

provided the integrals  $\alpha, \beta, \gamma$  have the same signification for an external point as they previously had for an internal one. In the case under consideration, of course

$$\gamma = 0.$$

If we assume

$$Q = 2nk\mu Ka \int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} \int_{c-\delta}^{c+\delta} \frac{\eta'}{\rho'(\rho' + \xi')} \frac{d\xi' d\eta' d\zeta'}{r}, \quad (39)$$

and

$$R = - \frac{\partial Q}{\partial \xi} = -2nk\mu Ka \int_{-\infty}^{+\infty} \int_{-\infty}^{+\infty} \int_{c-\delta}^{c+\delta} \frac{\eta'}{\rho'(\rho' + \xi')} \frac{\xi' - \xi}{r^3} d\xi' d\eta' d\zeta', \quad (40)$$

then, since  $\frac{\eta}{\rho(\rho + \xi)}$  vanishes at infinity, we have, by partial integration,

$$\begin{aligned}
\alpha &= 2nk\mu Ka \int \frac{\frac{\eta'}{\rho'(\rho' + \zeta')}}{\frac{\partial \eta'}{\partial \xi'}} \frac{d\xi' d\eta' d\zeta'}{r} \\
&= 2nk\mu Ka \int \frac{\eta'}{\rho'(\rho' + \zeta')} \frac{\eta - \eta'}{r^3} d\xi' d\eta' d\zeta' = \frac{\partial Q}{\partial \eta}, \\
\beta &= -2nk\mu Ka \int \frac{\frac{\eta'}{\rho'(\rho' + \zeta')}}{\frac{\partial \xi'}{\partial \xi}} \frac{d\xi' d\eta' d\zeta'}{r} \\
&= 2nk\mu Ka \int \frac{\eta'}{\rho'(\rho' + \zeta')} \frac{\xi - \xi'}{r^3} d\xi' d\eta' d\zeta' = -\frac{\partial Q}{\partial \xi};
\end{aligned}$$

hence

$$\left. \begin{aligned}
L' &= m \frac{\partial^2 Q}{\partial \xi \partial \zeta} = -m \frac{\partial R}{\partial \xi} = -m \frac{\partial R}{\partial x}, \\
M' &= m \frac{\partial^2 Q}{\partial \eta \partial \zeta} = -m \frac{\partial R}{\partial \eta} = -m \frac{\partial R}{\partial y}, \\
N' &= m \left( \frac{\partial^2 Q}{\partial \xi^2} + \frac{\partial^2 Q}{\partial \eta^2} \right) = -m \frac{\partial^2 Q}{\partial \zeta^2} = -m \frac{\partial R}{\partial z}.
\end{aligned} \right\} \quad (41)$$

Accordingly  $R$  represents the potential of the magnetic distribution by which, so far as concerns its action on a magnetic pole external to the conductor, the induced current-system may be replaced.

I reserve for publication elsewhere the comparison of the results obtained in the foregoing memoir with the present known observations. I propose then to consider the modifications to which these results will be subject from the circumstance that, with greater velocities of rotation, the inductive actions between the several parts of the disc become appreciable.

Berlin, November 1863.

## LXXVII. On the Motions of Eugenic Acid on the Surface of Water.

By CHARLES TOMLINSON, F.C.S., Lecturer on Science, King's College School, London\*.

[With a Plate.]

**W**HEN a drop of eugenic acid (the chief constituent of oil of cloves) is placed on the surface of water, it behaves much in the same manner as a fragment of camphor under similar conditions.

The continuous motions of a fragment of camphor on the

\* Communicated by the Author.

Fig 1

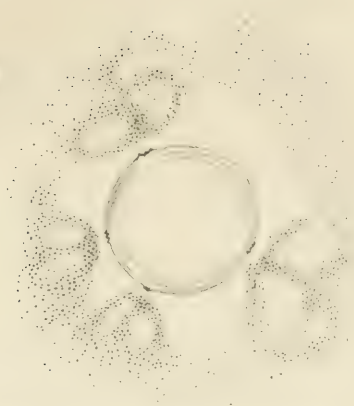


Figure 1. Les deux extrémités de la queue.

Fig 2

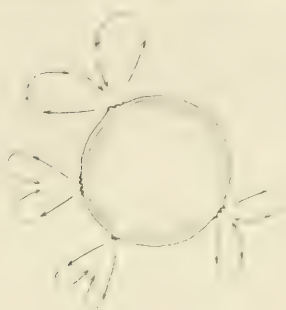
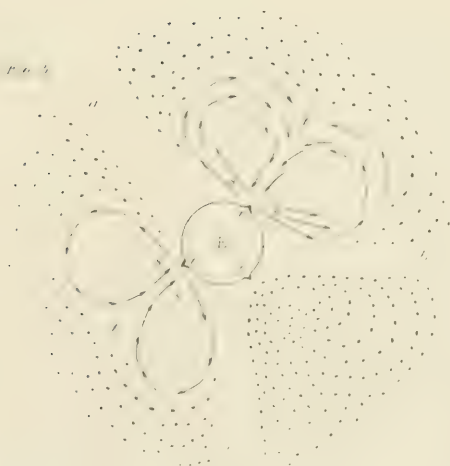


Fig 3







surface of water seem to depend (1) on the *formation* of a film of camphor which is detached from the fragment by the adhesion of the water; (2) on the *reaction* of such film on the fragment; (3) on the *evaporation*, and (4) on the *solution* of the film. In this way a film is always being produced and always being disposed of; but this incessant formation of the film which produces the recoil of the fragment, and also the evaporation of the film, are limited to the surface, while the whole body of the liquid is concerned in its solution. The motions of the fragment are retarded or arrested (1) by impurity, especially a slight greasiness on the water, whereby adhesion between the surface of the water and the camphor is prevented; (2) by the presence of damp air, or by covering up the vessel, whereby the evaporation of the film is prevented; (3) by saturation of the water, whereby the formation of the film is prevented.

This liberation and spreading of the camphor-films over the surface of the water, may be shown in a striking manner by causing a stick of camphor to dip into water over which lycopodium powder has been lightly dusted. The moment the camphor touches the water there is a wide repulsion of the powder, and presently a series of currents set in from the centre to the circumference, throwing the powder into the form of pairs of wheels, the members of each pair revolving in opposite directions. This action may in dry warm weather be kept up for days together\*.

If the stick of camphor be brought down into a large shallow vessel of water, such as a glass dish 5 or 6 inches in diameter, the surface of the water will, by the liberation of these films, be thrown into a state of vibration. At first as many as 250 pulsations may be counted in a minute, but as the water becomes saturated, the number declines to 60 or 70. As the action continues, regular ripple-marks are cut into the camphor in deepening lines, at and about the level where the air and the water meet†. Under favourable conditions the water will ascend and wear away the camphor as much as  $\frac{1}{10}$ th of an inch above the common level, and be depressed as much, the ascent and subsequent descent forming one slow pulsation.

The above experiments refer to common refined laurel camphor. Crude camphor, from which it is prepared, rotates well on the surface of water, as do also Borneo camphor, turpentine camphor, camphoric acid, solution of camphor in sulphuric acid, in benzole, chloroform, &c. Fragments of succinic, of benzoic, or of citric

\* These effects are fully described in a small volume entitled "Experimental Essays" published in 1863.

† The formation of this incision was first pointed out by Venturi in 1799 (*Ann. de Chimie*, vol. xxi. p. 262).

acid, and of the butyrates of baryta, lime, and magnesia, rotate well on water; and even inert substances smeared with an essential oil do the same,—the main condition being the formation and diffusion of a film on the surface of the water in one direction, so as to produce a recoil of the solid in another direction.

In most of the above cases the film is not visible, except by its effects on the lycopodium powder; but in other cases the film may be made quite evident, as when fragments of paper smeared with an essential oil, or little coracles of paper filled with oil, are set floating on water. The film diffusing from the paper in one direction produces motion in the opposite direction. A bit of solid oil of aniseed produces similar effects, visible to the eye and leaving no doubt as to the truth of the recoil theory in accounting for these motions.

There are certain liquids which also rotate on the surface of water. In general a drop of one of these liquids placed on water has an equal action from the centre outwards in all directions, sufficient to throw the surface of the water into vibration, but the conditions necessary to rotation are absent. Thus a drop of creosote gently delivered to the surface of water from the end of a glass rod, will display a highly agitated cohesion-figure, sailing about over the surface during some minutes\*. It often breaks up into separate disks, each of which forms a complete cohesion-figure; and as these become disposed of by solution, or separate into still smaller disks, the motions become more and more rapid. If the action is not equal all round, the disks begin to rotate with great velocity on a vertical axis until they disappear. This is especially the case if a small mote or fragment from the duster be on the surface; the small disk appears to seize this as a sort of lever, and whirls round with it with great rapidity—the real action, however, being that the mote protects a portion of the edge from diffusing out upon the surface, while the other portions do so with vigour, and in this way the retro-active condition necessary to rotation is established.

A drop of creosote, of carbolic acid, of oil of cloves, or of pepper, and also of eugenic acid, form cohesion-figures on water consisting in each case of a flattened disk about  $\frac{1}{10}$ ths or  $\frac{2}{10}$ ths of an inch in diameter, the edge of which is in a peculiar state of rapid vibration, caused by the struggle that is going on between the adhesion of the surface and the cohesion of the disk. The greater or less vigour of this action points out the difference between the figures of these liquids to the eye, while the respec-

\* This is more particularly described in a paper "On the Action of Vapours on Films," &c., contained in the *Philosophical Magazine* for August 1861, and on "Cohesion-Figures," in the Number for October of the same year.

tive durations establish differences capable of numerical expression. Thus, while a drop of carbolic acid on the surface of 2 ounces of water has a very vigorous action which lasts less than one minute (during which the disk diminishes in size until it has disappeared), a drop of creosote has a less vigorous existence which lasts five minutes, a drop of eugenic acid ten minutes, and a drop of oil of cloves or of pepper still longer. In all these cases a second drop placed on the water which received the first, has a longer and less energetic existence, because the adhesion of the water is partly satisfied. The duration of the first drop also varies with the state of the weather, and the age and exposure of the specimen.

Dr. Gladstone was so kind as to furnish me with a specimen of eugenic acid ( $\text{C}^{10}\text{H}^{12}\text{O}^2$ ), and I cannot help thinking that some of the results obtained with it are worthy of notice.

A drop of this acid was placed on the surface of 2 fluid ounces of distilled water contained in a glass capsule  $2\frac{1}{2}$  inches in diameter. It formed a good lively figure, but quickly split up into three disks, which in the course of two minutes ceased to vibrate. They soon recovered their vigour and began sailing about and revolving round each other; as they became smaller they increased in vigour, and at length disappeared in wild gyrations. A second drop placed on the same surface formed a disk which immediately split into two portions; these were active for the moment, and then became quite still. After some minutes they became active, not after the fashion of the first drop in sailing about, but in slowly rotating on a vertical axis, as was evident by watching one or two (resinified?) projecting points that had apparently grown out of the edge of the disk. Each of these points became the centre of a series of attractions and repulsions of the delicate silvery scales that had been left on the surface by the first drop. The scales apparently sailed up in regular lines to each point, where they divided and turned aside right and left\*. On dusting a little lycopodium powder over the surface of the water, the particles immediately formed currents about the eugenic-acid disk, they being apparently attracted by the pro-

\* The figure produced is very much like that given in my paper "On the Electrical Fly" (Phil. Mag. vol. xxvii. Pl. II. fig. 6), where metallic particles in turpentine are attracted and repelled by the wires connected with the machine and the earth. There is no reason to suppose that the eugenic-acid attractions and repulsions are electrical in their origin, although electricity may be a secondary result. I tried the experiment, in a metal dish, on the plate of a gold-leaf electrometer, and found that when the leaves are diverged either + or -, there is a trembling motion accompanying the action of the eugenic-acid disk or of creosote &c. An erking motion of the leaves is also produced when a film of oil spreads out on the surface of the water. But my apparatus is not sufficiently delicate for such experiments.



jecting points of the disk and repelled at the smooth parts. (See Plate VI. figs. 1 and 2). As the disk became smaller, it got entangled with some particles of the powder, and spun round with great velocity, very nearly like a small fragment of camphor on water. The larger disk also broke up and the separate portions moved over the surface with vigour. The lycopodium was powerfully driven away in front of the advancing disk, while other portions were dragged into its wake. It had occasional intervals of repose, during which the currents set in in regular order. The experiment had now lasted two hours, and still these alternations of activity and repose were maintained. The various disks in their wanderings always avoided each other, and in throwing off separate portions, these formed independent disks, while the parent disk thus lightened would go wandering about as if in search of something, and then, as if it had found a fixed axis, would whirl round with great rapidity, at the same time throwing off small fragments which, of course, whirled still more quickly. At length, as if weary of this rapid work, the disks became quiet, and remained so for many minutes. It often seemed as if a loud sound, a railway whistle, or a noise in the room, set the disks spinning again.

This second drop lasted  $2\frac{1}{4}$  hours. A third drop produced a powerful repulsion of the lycopodium particles, as in the case of camphor, and in like manner there was a pause, and then a setting in of currents with apparent attractions and repulsions. The disk soon began to spin on a vertical axis, and then to make wide sweeps over the surface. After five hours the disk was still moving slowly about, but the next morning it was quite motionless. The water was run through a filter, and returned to the capsule. Another drop of eugenic acid (the fourth) was placed on the surface, and fresh lycopodium powder was dusted on; there was a very feeble action. The water was evidently saturated; and to prove that the motions described are mainly due to solution, a small quantity of water was added to the saturated solution, when the disk split up, the lycopodium currents set in with vigour, and the smaller disks went whirling about as before. After some hours, saturation was again attained, and a fresh drop of eugenic acid was perfectly passive.

To try the effect of an enlarged surface, the same quantity of water (2 oz.) was poured into a shallow vessel 4 inches in diameter (the capsule being only  $2\frac{1}{2}$  inches). A drop of eugenic acid on this surface split up into numerous active disks, which disappeared in 11 minutes, while in the capsule the duration was 30 minutes. A second drop produced a much less active disk; it sailed slowly about for an hour, then suddenly became remarkably vigorous, and disappeared in another 15 minutes.



A third drop produced active currents in lycopodium powder. A fourth drop added next day was inert until fresh water was added, when it became active, as in the former case.

The camphor rotations are checked or stopped by any process which diminishes the adhesion of the water; so also in the case of eugenic acid, its motions are diminished if a soluble salt be added to the water. On a saturated solution of common salt the disk was contorted at the edge and then slowly rotated and moved about, driving before it the lycopodium powder. In about an hour the motion was over, and after 7 hours the disk still remained. The addition of distilled water to the solution did not restore the motion. The addition of 10 drops of sulphuric acid to 2 ounces of distilled water had a similar retarding effect on the motions of the disk of eugenic acid.

The action of small portions of oil placed on the water has a similar effect on the motions of eugenic acid as on those of camphor. I have already shown\* that while a fixed oil permanently arrests the motions of camphor, by forming a permanent film on the surface, and so preventing adhesion, a volatile oil, such as turpentine, only arrests the motions during its evaporation; or if newly distilled does not arrest them at all, the fragments of camphor skating from the water through the oily film and back into the water indifferently. The rotating disks of eugenic acid behave much in the same manner. When these disks do not prevent the drop of oil from spreading, which is often the case, they repel the film or cut their way through it with the greatest ease; and when the film is so far spent by solution and evaporation, or changed by the action of the air, the particles that are left on the surface form currents with the eugenic-acid disk, after the manner of the lycopodium powder already described.

The presence of the various oils on the surface of the water, whether as lenticules or films, has a remarkable influence on the duration of the eugenic-acid disk; for whereas the eugenic-acid disk on the surface of 2 ounces of distilled water in a glass capsule is about 10 minutes†, it may be an hour, or several hours, or even days, if a drop of one of the essential oils be

\* Philosophical Magazine for September 1863, "On the Action of Oils in arresting the Motions of Camphor on Water."

† The duration was much less than this when I first received the specimen from Dr. Gladstone. Another specimen of eugenic acid (quite colourless), prepared by my friend Mr. Hatcher, used the day after it was distilled, was very energetic in its motions, producing currents in the lycopodium particles at a considerable distance from the disk. The duration of the first drop on 2 oz. of distilled water in a glass capsule was 8 minutes, of the second drop 51 minutes, of the third drop 135 minutes. The fourth drop had not disappeared after 43 hours.

present. The pure hydrocarbons of these oils have a much less retarding effect, and the more volatile ones scarcely prolong the duration of the eugenic acid. When a drop of Persian naphtha was placed on the same surface with the eugenic-acid disk, the duration of the latter was extended to 20 minutes; with oil of cajuput 35 minutes; with essential oil of nutmeg 52 minutes; with paraffin oil 90 minutes; with oil of pepper 180 minutes; and with oil of lavender the eugenic acid had not disappeared after two days, although the 2 ounces of water were increased to 4 ounces on the second day.

In such cases the duration of the eugenic acid is but small if the oil placed by the side of it is very volatile and but little soluble in water, as in the case of some of the pure hydrocarbons of the essential oils. Some of the oils themselves form films and prevent contact between the eugenic-acid disk and the water; in such case solution is no longer possible, and the acid disk has an indefinite duration as in air. In other cases—with oil of lavender, for example, where a film is formed, a resinifying action sets in, and not only is the disk of eugenic acid displaced, but its resinification is apparently accelerated. Hence its remarkable duration.

To describe the behaviour of each oil that was tried on water in the presence of the eugenic-acid disk, would occupy too much space; but I may note down a few. Of course for each observation a clean capsule and fresh water (2 oz. of distilled) were used. While the eugenic acid was in vigorous action on the water, the surface was touched with a drop of oil of turpentine hanging from the end of a glass rod. The turpentine flashed into a film that covered the whole surface, but it entirely avoided the eugenic-acid disk. The latter was instantly struck motionless, and formed a well-shaped double convex lens; but it had a clear annular space around it free from the turpentine film. In a few minutes, however, the acid disk resumed its activity and invaded the turpentine film, cutting its way through it, while the resinified particles of the turpentine soon formed currents with the disk. When a drop of cubebs hydrocarbon was placed on the surface it did not spread, but formed a flattened disk, and immediately stopped the pulsations of the acid disk, which was at least an inch away from it. The acid disk sailed about, and by its influence indented the cubebs disk so as to make the latter kidney-shaped, a result not uncommon when the oils flatten down into disks. A fragment of camphor will do the same, as I have already noticed\*. In a few minutes some remarkable twitchings came over the eugenic-acid disk, and active pulsations were resumed. The disk then boldly attacked the cubebs disk,

\* In my "Experimental Essays" a figure representing this effect is given.

cutting it through and through without touching it until it (the acid disk) disappeared. A drop of oil of nutmeg on the water flashed out into a film, completely enclosing the eugenic-acid disk, not touching it, but stopping its motions. The acid disk soon resumed its activity, and shot out small portions of its substance, which skated about in the film with amazing rapidity and disappeared by solution. The acid disk split up into several disks, some of which played about in the film. As this broke up, it formed a large number of small oily-looking disks about the size of a small pin's head. These formed active currents with the eugenic disks, behaving in fact just like the lycopodium powder under similar circumstances. A drop of oil of bitter almonds did not spread at all, but formed a small well-defined lens, which was driven about with vigour by the eugenic-acid disk.

When a multitude of small fragments of camphor are busily gyrating on water, a drop of eugenic acid gently delivered to the surface arrests their motions; they become quite inert, and the acid disk makes use of them to form currents, as if they were particles of lycopodium powder.

Another point of comparison between the eugenic-acid disk and camphor on water is the influence of the air or of evaporation, which in the case of camphor is of even more importance than solution. On a damp day the camphor rotates sluggishly or not at all. The motions are also arrested if the vessel be covered up. It is impossible to produce the beautiful lycopodium currents in a large bottle; but with the same materials freely exposed to air, the currents are very vigorous, and may be kept up for hours or days\*. In such a case the water gets rid of its camphor by evaporation, and the air of the room becomes strongly scented with camphor. Evaporation does not seem to have much effect in maintaining the eugenic-acid motions. The disk will rotate and the lycopodium currents will go on in a stoppered bottle or in a narrow vessel whether exposed to the air or covered up; so that solution seems to be the chief force concerned in the eugenic-acid rotations and currents. The high boiling-point of eugenic acid is also favourable to this view.

The motions and currents produced by eugenic acid on the surface of water may also be noticed in a feebler form if a drop of oil of cloves or of oil of Jamaica pepper be gently deposited

\* These currents are produced, as already noticed, by lowering a stick of camphor vertically into water dusted faintly with lycopodium. If a piece of filtering paper be tied round a glass rod and the paper be slightly wetted with eugenic acid in the form of a ring round the paper, and this ring be lowered to the surface of water dusted with lycopodium, the currents, in the form of revolving wheels, similar to those of camphor, may be produced, but their duration is comparatively short.



on the surface of water\*. On the surface of acetic acid a drop of oil of cloves is far more energetic than on water, on account of its greater solubility in that liquid. It forms a very beautiful figure, reminding one of the sun in a fog, darting out rays and surrounded by a halo. The experiment was performed in a watch-glass. Feeble currents may also be noticed when a drop of paraffin oil is placed on the surface of pyroligenous ether.

The motions that have now been described seem to be due to the contest that takes place between the adhesion of the water and the cohesion of the drop in a substance of small solubility and volatility. As in the case of camphor, creosote, and a few other substances, the contact of water detaches from the eugenic acid an invisible film which surrounds the disk and exerts a repellent action all round. This film immediately enters into solution, and gives place to other films, which are similarly disposed of. This annular film, constantly given off by the eugenic-acid disk, prevents oils from touching it, and produces repulsion in the lycopodium powder. The first drop of acid is thus got rid of, and the adhesion of the water is so far impaired that, soon after the second drop is placed on the surface, the disk forms around it not an annular film, but portions only of a film, which spread out from the disk in broad lines or channels (*a, b, c*, fig. 3). Besides this, the second drop is exposed to the air so much longer than the first that partial resinification sets in at two or more points in the circumference of the disk. We have now the conditions favourable for the apparent attraction and repulsion of the particles. For if *E* (fig. 3) be the eugenic-acid disc, *a, b, c* the portions of film given off by it preparatory to entering into solution, and *rr* resinified points in the circumference of the disk, the radial portions, *a, b, c*, being constantly given off will repel the particles of lycopodium powder and drive them into the tranquil space *t*, where they will be constrained by other particles behind them to move forward towards the point *r*, but at and about this point they will be dragged into the currents formed by *ss*, and be again carried forwards or repelled in the direction of the arrows.

I have spoken of resinified points in the edge of the disk as being necessary to the formation of the circulating currents. If resinified, the points are inert; and any other inert substance ought to produce the same effect. A few specks of lycopodium powder at the edge answer very well. Thus in fig. 2, where the black specks represent particles of powder, currents are established as before, the flow of the particles towards the oil-disk not being the result of any attraction, but simply of a repulsion of the sur-

\* The oil of pepper used by me was obtained from the Jamaica Court of the International Exhibition.



face of the water in other directions. Thus, if currents be set up outward from a point in the direction of the straight arrows, there will be an *indraw* to the point as shown by the curved arrows. The surface water being driven away in two directions, other surface water must flow in to supply its place. Sometimes the powder rotates in complete circles or eddies. In such a case it is constantly tending to sink in the lower water by the side of a current which tends to carry it forward. These are precisely the conditions necessary to the formation of an eddy.

King's College, London,  
May 5, 1864.

LXXVIII. *On the Determination of Nitrogen by Weight.* By  
WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University\*.

**B**UNSEN† has given a method of analyzing nitrates and nitrites which renders it possible to determine all the constituents of the salt in a single analysis. This method consists essentially in igniting the salt in an atmosphere of nitrogen gas, absorbing the oxygen evolved by metallic copper, and collecting the water in a chloride-of-calcium tube. The nitrogen in the salt is given by the loss of weight in the apparatus.

In those analyses of nitrates or nitrites in which it is only desired to determine the nitrogen, the following process may be employed with advantage. A hard glass tube, about 6 inches in length, is sealed at one end, and its volume determined by filling it with mercury and pouring this into a graduated vessel. The tube is to be carefully dried and weighed with a good cork; it is then to be filled with finely-divided metallic copper, prepared by reduction of the oxide, so as to enable the operator to judge of the quantity necessary. The salt to be analyzed is then weighed and mixed with the metallic copper, either in a mortar or with a mixing wire in the tube, and the tube with its contents and cork is again weighed. The weight of the copper employed is thus known, and its volume may then be found by dividing this weight by the density of metallic copper. A weighed chloride-of-calcium tube is then adjusted as in organic analysis, and the combustion-tube is heated in the usual manner. When the combustion is finished, the open end of the chloride-of-calcium tube is sealed with the blowpipe flame and the combustion-tube allowed to become perfectly cold. The chloride tube is then removed and weighed, and the combustion-tube

\* From Silliman's American Journal for May 1864.

† *Ann. der Chem. und Pharm.* vol. lxxii. p. 40.

also weighed with its cork. The increase in weight of the chloride-of-calcium tube gives the amount of moisture in the copper and the water in the salt analyzed. The loss of weight in the combustion-tube gives the nitrogen in the salt after correction for the oxygen in the tube, for the moisture in the copper, and for the water in the salt. The correction for the oxygen in the combustion-tube absorbed by the copper is easily found, with a sufficiently close approximation, by subtracting the volume of the copper from that of the tube, finding the weight of the residual air, taking one-fifth of this as oxygen, and considering the whole of this oxygen as absorbed by the copper. A piece of asbestos may be placed between the copper and the cork with advantage, but this renders an additional correction necessary. Two analyses were executed by this method. In the first a sample of pure saltpetre gave 13·86 per cent. nitrogen; the formula  $\text{KO}, \text{NO}^5$  requires 13·86 per cent. In the second a specimen of the commercial salt gave 13·7 per cent. nitrogen, while the same salt analyzed by Simpson's method, in which the volume of the nitrogen is determined, also gave 13·7 per cent. The whole analysis, with the weighings, may easily be executed in an hour and a half by a single person. It is easy to see that this method applies to all inorganic nitrates and nitrites, whether hydrous or anhydrous, but that it cannot be employed in the case of organic or ammoniacal salts. In the analysis of inorganic nitrates or nitrites by Simpson's method, it is not necessary to use oxide of mercury to prevent the formation of deutoxide of nitrogen. In all such cases it will be found sufficient to mix the salt with pure metallic copper. In this manner the dimensions of the combustion-tube may be greatly diminished. I have also found it advantageous to pump out the air from the combustion-tube by a small hand air-pump before disengaging carbonic acid from the carbonate of manganese. By alternately pumping and filling the tube with carbonic acid, the air may be completely expelled before the combustion commences. It is also better to draw the tube out before a Bunsen's gas-blast, as it is difficult to make a cork and india-rubber connector perfectly tight. With a little practice the drawing out is easily effected, even with the hardest combustion-tubes. Where many nitrogen-determinations are made, it will be found convenient to employ printed forms for logarithmic calculation, the logarithmic constants of reduction being printed upon the form itself in their proper places.

LXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 477.]

June 18, 1863.—Major-General Sabine, President, in the Chair.

THE following communications were read:—

“Some Remarks appended to a Report on Mr. Hopkins’s Paper ‘On the Theory of the Motion of Glaciers.’” By Sir John F. W. Herschel, Bart., F.R.S.

A few remarks arising out of the perusal of this paper may perhaps not be considered as out of place on the present occasion. They are not meant as in any way impugning the author’s views of the laws determining the fracture and disruption of glacier masses, or their application to glacier-phenomena in general, but in relation to the somewhat mysterious process of regelation itself, and to those generally recognized and most remarkable facts of the gradual conversion of snow into more or less transparent ice, and the reunion of blocks and fissured or broken fragments, under the joint influence of renewed pressure and of that process (whatever its nature), into continuous masses. If regelation be really a process of crystallization, it seems exceedingly difficult to imagine how the molecules forming the cementing layer between two juxtaposed surfaces can at once arrange themselves conformably to the accidentally differing axial arrangements of those of the two surfaces cemented. A macled crystal is indeed a crystallographical possibility; but then the axes of the two individuals cohering by the macle-plane have to each other a definite geometrical relation in space, as is well exemplified in the case of the interrupting film in Iceland spar. At the temperature at which “regelation” takes place (*viz.* the precise limit between the liquid and solid states), it seems to me very possible that the cohesive forces of the molecules of the cemented surfaces may be so nearly counteracted as to bring those surfaces into what may be so far regarded as a viscous state as to permit (not indeed a sensible and finite change of figure of a small portion of the mass without fracture, but) a certain freedom of movement in the individual molecules, to some sensible depth within the surface, so as to allow of a *gradually progressive* deviation of *their axes* from exact parallelism, and thus to effect a transition from one crystalline arrangement to another—not by macleing, but by curvilinear distortion, such as may be conceived to prevail in pearl-spar and other similar disturbed forms of crystals. Nay, I can conceive it possible that by very long continuance at this exact temperature (especially if aided by tremors short of disruption propagated through the mass, which, as we see in the crystallization of cold wrought iron in the axle-trees of railway carriages, powerfully favour the crystalline rearrangement of molecules even in the most rigid solids) the contiguous blocks may influence each other’s crystallization to a greater and greater depth through the medium of the cementing film, thus tending continually to straiten the curve of the connecting chain of axes, and after a very long time to bring the two blocks into perfect conformity, so as to form an uninterrupted



crystal; and this, or something like this, I take to be the process by which the snow of a *nevé* is converted into the imperfectly transparent and sometimes fully transparent ice of a glacier. Tremors of the kind here alluded to would not be wanting in a glacier in continued process of displacement, and in some part or other of which disruptions consequent on violent strain are momentarily taking place.

On the subject of the temperature of the interior of a glacier, I would observe that there will be found in the archives of the Royal Society, on the occasion of the Committee for recommending objects of inquiry to Lieut. Foster during one of his Polar expeditions, a recommendation of mine that the expedition should be furnished with a set of boring-implements for the purpose of piercing some very large and compact mass of ice, with the expectation of finding it much below the freezing-temperature. The heat of summer, it was suggested, would all be carried off in the water resulting from surface-melting; while the intense cold of a polar winter would penetrate the interior, and thus give rise to a *mean* temperature very far below that of the external climate. The implements (if I remember rightly) *were* furnished, but Lieut. Foster reported that no mass of ice sufficiently large could be met with so free from fissures as not to be permeated by infiltrating water during the summer months; and if any results were obtained, they were not striking or definite enough to be worth recording. That the lower surface of a glacier in contact with the earth is in a constant state of fusion, even in those cold regions, is proved by the phenomena recorded by Dr. Kane of the Mary Minturn River in lat.  $78^{\circ} 54'$ , and the feeder of the Kane Lake, lat.  $78^{\circ} 18'$ , which never ceases to flow, summer or winter. Admitting this as a general fact, the sliding of a glacier on its bed is an obvious necessity; and that it should be unaccelerated is no more a matter of wonder or difficulty of conception than the unaccelerated descent of the weight of a clock which is never abandoned to its own impetus, but brought to rest after every momentary descent by the action of the scapement,—or the unaccelerated fall of a body in a resisting medium when the resistance becomes equal to gravity,—or of a weight gradually and uniformly lowered by the hand. Perhaps the more general way of conceiving it would be to regard the whole glacier as a mass propped up against a support anyhow inclined, and prevented from tumbling over sideways by lateral stays. Such a mass would rest in its position, if duly supported either by a base-abutment, or by a heap of its own *débris*; but if these were slowly abraded, destroyed, or picked away, the whole mass would descend bodily in the exact manner of the withdrawal of support.

On the disruption of a nearly homogeneous elastic solid in a state of strain, I would add a remark which seems to me of some moment, as explanatory of the greater cohesive strength which is well known to be imparted to cements, especially those of a resinous or gummy nature, by the admixture of extraneous matter in fine powder. If in such a solid there be one portion, however small, weaker than the rest, the strain being uniform, a crack will originate in that place. Now a crack, once produced, has a tendency to run—for this plain reason,



that at its momentary limit, at the point on which it has just arrived, the divellent force on the molecules there situated is counteracted only by half the cohesive force which acted when there was no crack, viz., the cohesion of the uncracked portion alone. But if a crack anywhere produced be stopped from running by encountering a solid particle of greater cohesive force, fracture will no longer be determined by the accidental deficiency of cohesion at some weak point, but by the average resistance of the whole cementing mass.

It would, I think, be interesting to determine, on the more transparent portions of glacier ice, by the simple and easily applied test of polarized light, whether a definite crystalline structure prevail in its interior, and if so, in what direction the axis lies in relation to the lines of fissure in the crevasses. Nor is there any reason why the idea above thrown out respecting the mutual modification of structure of two masses cemented by regelation, at or near their plane of junction, should not be subjected to a similar test.

December 10.—Major-General Sabine, President, in the Chair.

The following communication was read :—

“On the Spectra of some of the Chemical Elements.” By William Huggins, Esq., F.R.A.S.

The author has been engaged for some time in association with Prof. W. A. Miller in observing the spectra of the fixed stars. For the purpose of comparing the spectra of these with the spectra of the terrestrial elements, no maps of the latter were found that were conveniently available. Kirchhoff's maps and tables, besides their partial incompleteness, were not suited for night work when the sun could not be simultaneously observed.

The author adopts the lines of the spectrum of common air as the fiducial points of a standard scale to which the spectra of the elements are referred. The air-spectrum has the advantage of being always visible with the spectra of the metals without increased complication of apparatus.

The observations were made with a spectroscope of six prisms of heavy glass of large size. The total deviation of the light with this train of prisms is for the *D* ray about  $198^{\circ}$ . The telescope and the collimator have both an aperture of 1.7 inch. The focal length of the telescope is 16.5 inches. The measures were partly taken from the readings of a finely divided arc of brass, which the arm carrying the telescope traverses, and partly from the readings of a wire micrometer attached to the eye-end of the telescope. The scale of measurement adopted gives five divisions for the interval between the components of the double line *D*. The excellent performance of this instrument is shown by the great distinctness of the finer lines of the solar spectrum. All those mapped by Kirchhoff are seen, and many others in addition to these.

The spark of an induction coil was employed, into the secondary circuit of which a battery of nine Leyden jars was introduced. The Leyden jars are arranged in three batteries of three jars each, and the batteries connected in series.

The relative intensities and distinctive characters of the lines are represented by figures and letters, placed against the numbers in the Tables.

The spectrum, which extends from *a* to *H*, is divided, and forms two maps. The air-spectrum and the principal solar lines are placed at the top of each map, and below these the spectra of the following metals:—Sodium, potassium, calcium, barium, strontium, manganese, thallium, silver, tellurium, tin, iron, cadmium, antimony, gold, bismuth, mercury, cobalt, arsenic, lead, zinc, chromium, osmium, palladium, and platinum.

The lines of the air-spectrum are referred to the components of air to which they severally belong. An unexpected result was observed: two strong lines of the air-spectrum, one of them a double line, were seen to be common to the spectra of oxygen and nitrogen. These gases were obtained from different sources with identical results. The strong red line of the air-spectrum is shown to be due to the presence of aqueous vapour, and to coincide with the line of hydrogen. The carbonic acid in the air is not revealed by spectrum analysis.

Three pairs of lines and one band of haze are given in the sodium spectrum in addition to the double D line. As these might be due to impurities of the commercial sodium employed, the observation was confirmed by an amalgam of sodium prepared by the voltaic method from pure chloride of sodium. Two of these pairs of lines have been recognized in the spectrum of a saturated solution of pure nitrate of soda.

The two stronger pairs appear to agree in position with solar lines having the following numbers in Kirchhoff's scale:—864·4 and 867·1, and 1150·2 and 1154·2.

The spectrum from electrodes of potassium contains many new lines. For the spectra of calcium, lithium, and strontium, metallic calcium, lithium, and strontium were employed.

Barium was mapped from an amalgam of barium prepared by electricity from chloride of barium.

The following metals were employed in the form of electro-deposits upon platinum:—manganese, silver, tin, iron, cadmium, antimony, bismuth, cobalt, lead, zinc, and chromium. Care was taken that the other metals should be reliable for purity.

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#### GEOLOGICAL SOCIETY.

[Continued from p. 477.]

February 3, 1864.—Professor A. C. Ramsay, President, in the Chair.

The following communication was read:—

“On the Permian Rocks of the North-west of England, and their Extension into Scotland.” By Sir. R. I. Murchison, K.C.B., F.R.S., F.G.S., and Professor R. Harkness, F.R.S., F.G.S.

In this paper the authors propounded a new view of the composition of the Permian Group in the north-west of England, and, by the consequent rearrangement of the rocks involved in this change in classification, they were enabled to place the Permian strata of Great Britain in direct correlation with those of the continent of Europe. This new feature in British classification is the assignment of a large amount of red sandstone in the north-western counties to the Permian period, and its removal from the New Red Sandstone, or Trias-formation, to which they have hitherto been assigned in all geological maps. The authors showed that these red sandstones are closely and conformably united with the Magnesian Limestone or its equivalent, and form the natural upper limit of the Palæozoic deposits. They affirmed that thus a tripartite arrangement of the Permian rocks holds good in Westmoreland, Cumberland, and Lancashire, and that the three subdivisions are correlative with those formerly shown by Sir R. I. Murchison to exist in the Permian deposits of Germany and Russia, thus proving the inapplicability of the term Dyas to this group of rocks.

The difference in lithological details of the Permian rocks of the north-west of England from those on the opposite flank of the Pennine chain was next adverted to; and it was observed that, with so vast a dissimilarity in their lithological development in England, we need not be surprised at finding still greater diversities in these protean deposits when followed into Germany and Russia.

The discovery, by Professor Harkness, in the central member of this siliceous group in Westmoreland, of numerous fossil Plants identical with the species of the Kupfer-Schiefer in Germany, and in the Marl-slate of the Magnesian Limestone of Durham, was given as a strong proof of the correctness of the authors' conclusions.

The comparative scarcity of igneous rocks, and the evidence of powerful chemical action, in the Permian strata of Britain, is contrasted with their abundance in deposits of that age in Germany; but proofs are nevertheless brought forward to show that the hæmatite of Cumberland and Lancashire was formed in the early accumulation of the Permian deposits.

In describing in detail the different members of the Permian group of the north-west of England, the authors define the downward and upward limit of the strata which have undergone dolomitization; for whilst certain bands of calcareous breccia (the "brockrum" of the natives), which occur in the central portion of the series, contain much magnesia, the lower breccias, composed of the same mountain-limestone fragments, have no trace of it; nor is it to be detected in the upper member, or St. Bee's Sandstone.

February 24.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On further Discoveries of Flint Implements and Fossil Mammalia." By J. Wyatt, Esq., F.G.S.

The opening of a section at Summerhouse Hill gave the author



an opportunity of ascertaining whether the gravels at that lower level exhibited any features different from those of the upper level at Biddenham. Although, as might have been expected, some of the species of mammals were found to be common to the two localities, yet that under notice furnished some species of mammals, as well as of land and freshwater shells, together with a few types of flint implements, differing from those met with at higher levels.

Mr. Wyatt described the section at Summerhouse Hill in detail, showing that it tended to support Mr. Prestwich's opinions respecting the formation of gravel-beds; he also described the Flint Implements he had recently found, comparing them with known specimens from the Valley of the Somme and elsewhere; and he stated that he was now enabled to add two new localities near Bedford—Summerhouse Hill and Honey Hill—to those already known as having furnished similar weapons.

2. "On some Recent Discoveries of Flint Implements in Drift Deposits in Hants and Wilts." By John Evans, Esq., F.G.S., F.S.A.

Flint Implements having recently been found on the sea-shore about midway between Southampton and Gosport, by Mr. James Brown, of Salisbury, and also at Fisherton, near Salisbury, by Dr. H. P. Blackmore of that place, the author visited these localities in company with Mr. Prestwich, and gave the results of his observations in this paper.

After describing the implements from near Southampton, and having shown that their condition is identical with that of the materials composing the gravel capping the adjacent cliff, Mr. Evans proceeded to review the evidence of the great antiquity of these remains, which rested mainly on the circumstance that these gravel-beds, like those of Reculver, are of fluvatile origin, although now abutting on the sea.

In like manner the author then described the Fisherton implements, and the gravel-pits from which they were obtained. The relation of the high-level gravels (in which the implements were found) to the lower-level gravels of the valley of the Avon was next discussed, and the geological features of the former deposits particularly described, lists of the fossils (including the Mammalia and the Land and Freshwater Shells) being also given. Mr. Evans came to the conclusion that the fossils bore evidence of the climate, at the time when they were deposited, having been more rigorous, at any rate in the winter, than it now is; and to this cause he attributed the comparatively greater excavating power of the early Postpliocene rivers.

March 9.—W. J. Hamilton, President, in the Chair.

The following communications were read:—

1. "On the Discovery of the Scales of *Pteraspis*, with some remarks on the Cephalic Shield of that Fish." By E. Ray Lankester, Esq. Communicated by Prof. T. H. Huxley, F.R.S., F.G.S.

The successive steps by which the genus *Pteraspis* came to be



established, and the grounds on which the prevalent opinion as to its ichthyic nature rests, having been noticed, the author proceeded to describe in detail the scales which have lately been discovered at Cradley, near Malvern, and which alone were required to remove all doubt as to the affinities of the genus, comparing them with those of *Cephalaspis*, to some of which they bear a great resemblance; and he concluded by giving a description of the markings on the surface of the cephalic shield of *Pteraspis rostratus*.

2. "On some Remains of *Bothriolepis* from the Upper Devonian Sandstones of Elgin." By George E. Roberts, Esq.

Remains of a large Dendrodoid *Cœlacanth* obtained by the author in Elgin were referred by him to the genus *Bothriolepis*. These consisted of two large casts of a central head-plate, with portions of the test; a natural cast considered by him to represent the parietal, squamosal, scapular, and coracoid bones; casts of the nasal bones, and teeth of the upper jaw; together with tooth-like bodies, which were suggested to be teeth originally situated in the posterior region of the mouth.

The ornament borne upon the head-plate was next described by the author; and, in conclusion, the affinities between the genera *Bothriolepis*, *Asterolepis*, *Pteraspis*, and *Cephalaspis* were discussed.

3. "On Missing Sedimentary Formations, from Suspension or Removal of Deposits,—their general relations and importance." By J. J. Bigsby, M.D., F.G.S.

In this paper the author brought together nearly all the known instances of gaps or blanks in the stratigraphical succession in different countries. Of the two formations which are in apposition through the absence of one or more formations, which thus constitutes a blank, Dr. Bigsby applied the term "Roof" to the upper, and "Floor" to the lower. He then described briefly the principal instances, arranging them according to the age of the formation constituting the "Roof," and drawing certain inferences from a consideration of them respecting the influence of Oscillation of Level, Emergence, Denudation, &c., in their production.

In his concluding observations, Dr. Bigsby observed that these gaps indicate that there always have been areas of dry land, and also showed that they prove the geological record to be, in places, very much obscured, if not entirely obliterated.

## LXXX. Intelligence and Miscellaneous Articles.

### ON THE APPEARANCES OF THE SUN'S DISK.

BY C. G. TALMAGE, ESQ.

I SHOULD have ventured to communicate the contents of this paper to the Society at an earlier period, had I not felt some diffidence in saying that I have never been able to see those objects which are so positively declared to exist by persons who may be considered authorities on such matters; but when the same thing is stated by a gentleman who possesses the great experience and acute vision of

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the Rev. W. R. Dawes, I feel a little more confidence in giving the result of my own observations.

Since November 1861 I have most carefully searched for Mr. Nasmyth's "willow-leaves" under various circumstances.

From November 1861 to April 1862 I was engaged in astronomical pursuits at Nice, and on every fine day I examined the sun with one of three equatoreals—a 4-inch, a 6-inch, and an 8-inch (belonging to Mr. Coventry), with powers from 40 to 500. In the summer of 1862 I examined the sun at Paris with another 8-inch object-glass of exquisite definition. In November of the same year I revisited Nice; and until my return, in April 1863, I omitted no opportunity of searching for the "willow-leaves," but never have I seen the slightest trace of "willow-leaves," "rice-grains," or "thatch."

During this spring I have also examined the sun with a small telescope, fitted with a binocular eyepiece belonging to a first-class microscope, and without any greater success, though the spots are clearly shown as funnel-shaped depressions or holes: all that I see on the sun is merely a difference of greater or less luminosity.

A remark was made at our last meeting that perhaps these appearances depend on focus, and the examination of Diatomaceæ out of focus was given as an instance. When I returned home I examined several specimens of Diatomaceæ, both easily resolvable and tests (*Navicula cuspidata*), with various powers of from 3-inch to a  $\frac{1}{8}$ -inch, in and out of focus; and I could compare them to nothing but diatoms out of focus; there was not the least resemblance to anything I have ever seen on the solar disk.—*From the Monthly Notices of the Royal Astronomical Society*, May 13, 1864.

#### ON THE YELLOW COLORATION OF FADED PHOTOGRAPHIC PRINTS.

BY M. CAREY LEA.

Everything connected with the permanence of photographic products has an interest so vital to photography, that nothing connected with it can be considered as trivial. We are yet groping in the dark as to the causes of failure. "Sulphuration" is a convenient word, but it would be more satisfactory if we had some idea as to the nature of the obnoxious insoluble sulphur compound.

The hypothesis which has for some time past become current is, that the fading depends upon the presence in the print of some sulphur compound, which with time acts upon the silver, converting it, as is said, into sulphide. Sulphide of silver we know as a jet-black substance, and we habitually convert the reduced silver of negatives into sulphide when we wish to intensify them. Why then should the production of sulphide of silver strengthen a negative and destroy a print?

MM. Davanne and Girard answer as follows. Sulphide of silver prepared by itself is, they say, black; but formed in presence of organic matter, the organic matter enters into the composition and the compound is yellow. To prove this, they precipitate a solution of silver with sulphuretted hydrogen, and find it violet-black. They

again precipitate the same silver solution in presence of starch, and obtain a yellow precipitate.

I have paid much attention to the subject of the fading of prints, and had been forcibly struck by the anomaly above referred to. The explanation of the gentlemen just mentioned seemed very satisfactory; but the question appeared to have too much importance to pass it by without verification. The following results were obtained.

In a test-tube was placed a little boiled starch; in a second tube, some water. Two or three drops of a five per cent. solution of nitrate of silver were added to each, and then hydrosulphate of ammonia. *Both* solutions gave a yellowish-brown precipitate without the slightest difference in shade or colour. The precipitate afforded by hydrosulphate of ammonia could not here differ from that produced by sulphuretted hydrogen, but to remove any doubt on this score, the experiment was carefully repeated with sulphuretted hydrogen. The same result precisely was obtained. As a further confirmation, the experiment was varied by the substitution of another organic substance, viz. collodion, instead of the starch. The result was precisely similar.

The different result obtained by MM. Davanne and Girard can perhaps be explained as follows. The sulphide of silver is a heavy substance, and when thrown down from a simple watery solution it quickly subsides. But solutions thickened with organic matter, such as gum, starch, gelatine, &c., retain a precipitate for a long time in a state of suspension, thus preserving the original yellowish-brown appearance. If, therefore, we prepare solutions as above, and pass HS through them, allowing them to subside, and examine them only after a time, we shall find in the one case a black precipitate below a colourless solution, in the other a brownish viscid liquid. But if we watch the process from the outset, we shall see that the reactions are chemically identical, and differ only in respect to the mechanical suspension which takes place in the one case.

The experiment which I here cite may throw some light on the origin of the yellow colour. It appears that when the sulphide of silver is in a state of very fine division, its colour is yellowish brown, as may be easily ascertained by treating a very dilute solution (*e. g.*  $\frac{1}{5000}$ th) of nitrate of silver with hydrosulphate of ammonia. Many substances are only black in consequence of excessive intensity of colour. Lampblack, for example, in a state of excessively fine division is yellowish brown. Ink diluted is purple. Claus has shown that the intensely black hydrated sesquioxide of ruthenium is, when very finely divided, green.

I therefore conclude: 1st. That it is as yet not absolutely demonstrated (although probable) that the current opinion, ascribing the fading of pictures to the production of sulphide of silver, is correct.

2nd. That if this opinion be correct, there exists no evidence that organic compounds have anything to do with the production of a yellow colour, the tint of sulphide of silver found in their presence and in their absence being quite the same shade of yellow-brown.—Silliman's *American Journal*, May 1864.

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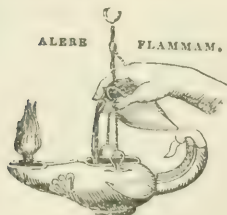
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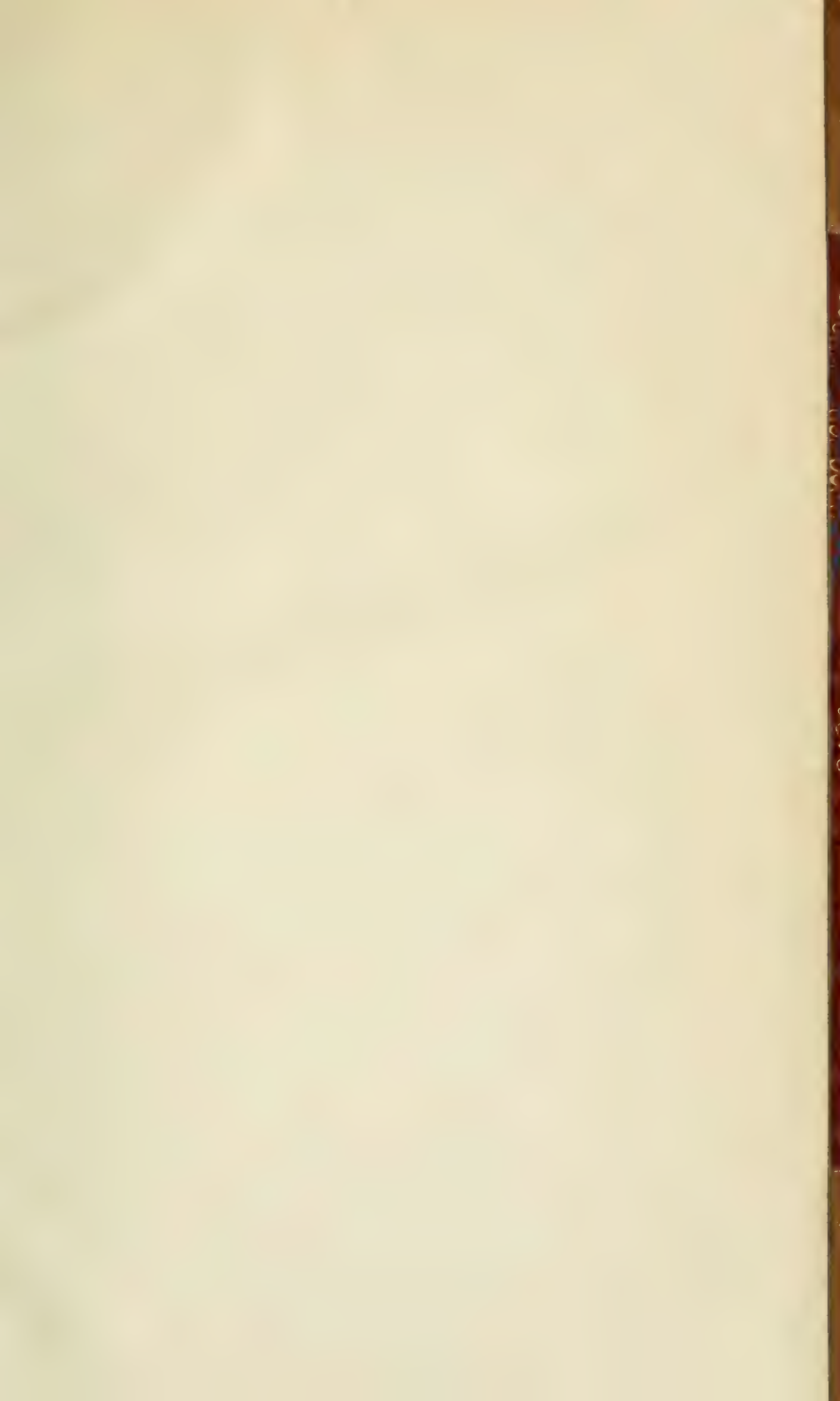
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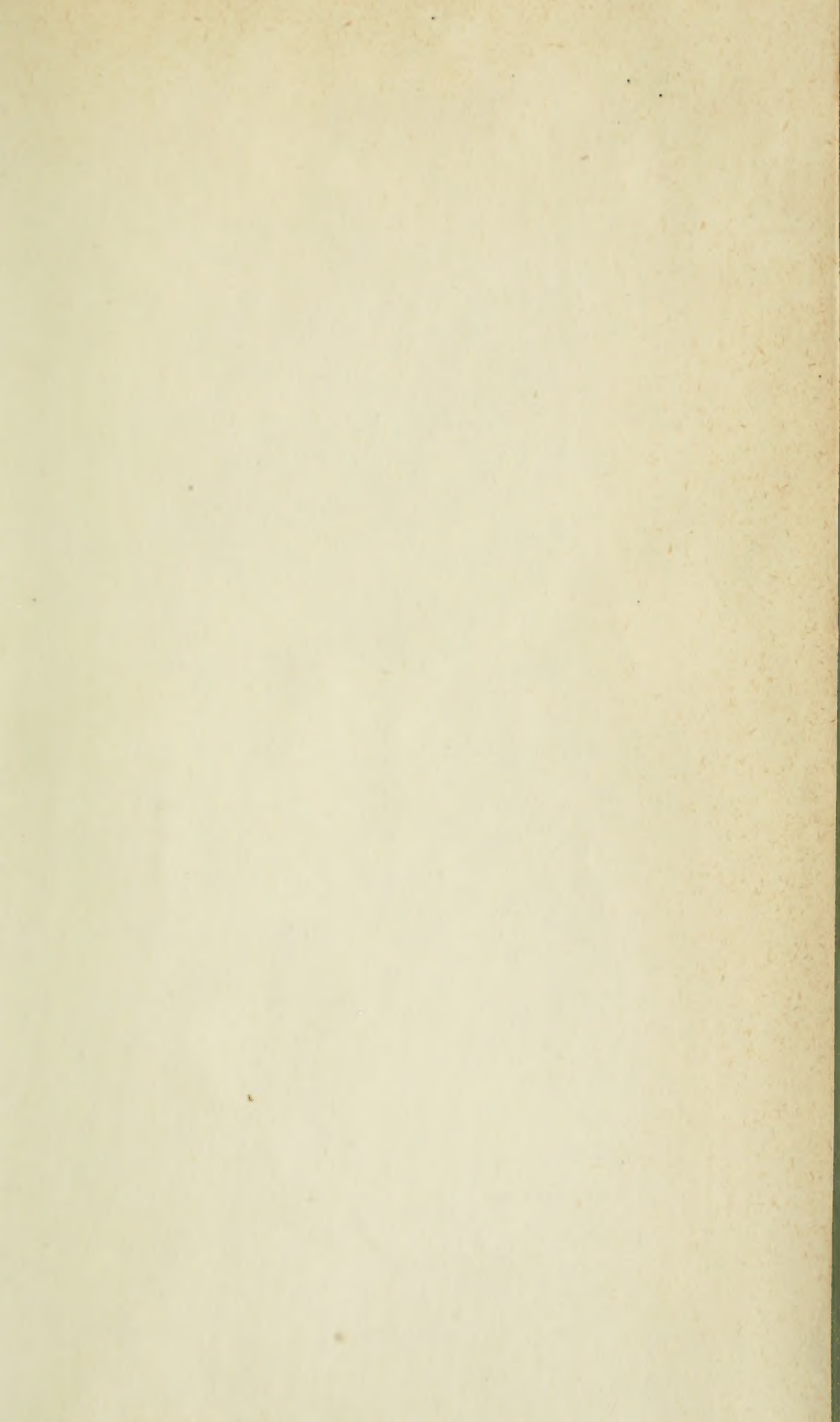
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